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ABSTRACT

This review of research literature explores the possible harmful effects of day care programs on children's health and physical development, motor development, cognitive development, and social and emotional development, and suggests how variations in staff/child ratios may influence these effects. Two broad categories of conditions are identified as being harmful to children: directed techniques in which the caregiver is being actively harmful and general sensory deprivation which can be active but often takes the form of benign neglect. Findings indicate harm is an elusive concept: children can be harmed physically, but psychological harm requires severe deprivation over extended periods of time before consequences become irreversible. Child/staff ratios are examined in relation to child characteristics, setting (family-style or center-based), program philosophy, caregiver characteristics, and level of standards (for minimal vs. optimal quality programs). Findings indicate that low child/staff ratios tend to produce quieter, less aggressive children who perform better on standardized tests. It is suggested that the Federal Interagency Day Care Requirements (FIDCR) should focus on specifying the actual number of children working with a caregiver, rather than an overall ratio of all children to all caregivers in a program. (CM)

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It is being distributed to solicit comments and criticisms to assure technical soundness, the adequacy of supporting materials, relevance, and general comprehensibility. It is hoped that this paper and its review will contribute to the improvement of the public debate on social policy and the proper Federal role in regulating child day care.

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Statement of the Problem

The Federal Interagency Day Care Requirements (FIDCR) define a set of regulations that operators must satisfy in order to receive federal subsidies. Congress raised questions about the FIDCR because of the differences between the federal regulations and the states' licensing codes. A general feeling existed that there was little scientific evidence to justify many of the federal standards and requested that the FIDCR be re-evaluated.

The purpose of this paper is to examine the scientific literature relevant to the effects of child/staff ratios on children's development. Corollary to this objective, the paper attempted to identify domains of behaviors that might be influenced as a consequence of a day care experience. A pervasive concern throughout the paper was an effort to determine what is meant by harm; that is, when can we state with certainty that a child has been harmed as a consequence of a particular component of a program? In this report exclusive attention is given to staffing patterns in family-style and center-based programs. The discussion of harm focuses primarily on cognitive development and socialization, especially aggression. The methodological and social/political issues which limit the implications of the research are explored.

Objectives

The major questions addressed in this position paper are: (1) What are the major categories of behavioral outcomes resulting from day care that should be of concern to policy makers (pp. 2-4; 10-12); (2) What is meant by harm, particularly psychological harm, and how can harm be assessed (pp. 12-52); (3) To what degree do variations in child/staff ratios influence behavioral outcomes (cause harm) (pp. 53-65); and, (4) Evaluate the FIDCR in terms of staffing regulations as they relate to the quality of behavioral outcomes.

Methodology

The major work in producing this paper was reading the relevant research literature. Location of relevent papers (staff/child studies) became a major burden because so few exist and most of them are unpublished. In all likelihood, many useful papers were missed. ERIC documents were examined and the NTIS was used; these efforts had little or no pay off. Studies that were available, and of reasonable scientific quality, focused on institutional care not day care; a major limitation. Each available study was examined to determine the chronological age of subjects, number of subjects in each group, socio-economic status of the subjects, the organizational structure of the program (how children were assigned to caregivers), quality of caregivers (educational levels), and the general philosophy of the programs, including whether the programs were family- or center-based. In very rare instances it was also possible to describe the programs actually received by the children.

Major Findings and Recommendations

Several aspects of the report can be considered major findings and will be examined at first separately and then in terms of interactions.

Evaluation of Research Methodology (pp. 3-10). Essentially there are no firm research studies on the basis of which firm conclusions about child/staff ratios can be made. Studies employ very unique samples, use non-random assignment procedures without quasi-experimental analytic techniques, fail to describe essential program variables, use children as the unit of analysis rather than the group (to study teacher effects), use evaluation based on very short time spans (9 months), use difference scores without regard to either regression effects or the effects of pretesting on the outcome measures, and, make broad generalizations based on differences at the 10 percent level of significance. In effect, relevant studies typically suffer from many of these defects and thus cannot be considered definitive studies.

The Concept of Harm (pp. 12-32). Much of the literature examining the antecedents of harm involves children reared from infancy in severely deprived environments (institutional or natural environment). As noted, these studies are not completely relevant to day care effects. The three major studies (Dennis' study of orphans adopted versus non-adopted children, Skeels' study of infants placed in an institution for mentally retarded, and Kagan's study of rural Indian babies) all indicate that long-term effects are minimal or even not apparent. All three investigators view their findings as showing that early deprivation effects can be ameliorated by a stimulating environment. In Kagan's study, the increased environmental stimulation occurred as the children were permitted to actively interact with the environment. Kagan is careful to note that the adult status of his subjects was commensurate with their peers; that is, he argues that adult outcomes (harm) are relative to specific cultures.

The general discussion of harm was mostly cast in the context of relative values. Thus, many researchers observe that children in center-based day care are more aggressive than children in family-style day care or who remain at home. These investigators suggest that the aggressiveness is a negative consequence of day care (most often day care infers mother-child separation). Studies of day care conducted 40 years ago, however, seemed to encourage aggression; socially acceptable aggression such as defending one's self from attack, speaking to adults, or maintaining property. Current authors fail to consider the positive aspects of aggression suggesting, to some degree, a change in values. That day care (mother-child separation) can be blamed for the observed aggression was seriously questioned. Studies were presented which clearly demonstrate that program variables (structure, space available, child/staff ratios) are the major contributors to variations in aggression as well as variations in performance on cognitive tasks.

A case was made that a formal educational component in day care was not necessary to prevent harm. A minimal program providing basically for the physical, social, and emotional requirements of children is an entirely defensible approach. The case was based on the fact that children can receive educational stimulation from their parents and siblings and that education in fact occurs without formal lessons. Data were presented showing that regardless of whether preschool children received formal structured educational programs or not, differences between the groups which existed in the preschool disappear at the end of third grade. Children are very versatile and they will use any of numerous ways of acquiring whatever the environment presents to them.

Summarily, harm proved to be a rather elusive concept, especially when an effort to document its occurrence was made. Clearly, children can be harmed physically in innumerable ways, but psychological harm seems to require severe deprivation over extended (perhaps 5-10 years) periods of time before the consequences become irreversible.

Child/Staff Ratios (pp. 53-63). Studies directly addressing the effects of child/staff ratios on development, unconfounded by innumerable other variables, do not exist. Inferences regarding child/staff effects are possible, however, by examining the consistencies that occur over diverse studies (this strategy would be more valid if more studies existed). Since the child/staff findings are central to this paper, each major inference will be treated separately followed by a brief summary.

1. Low child/staff ratios (2/1, 3/1, or 4/1) tend to produce children who are quieter, less aggressive, and who perform well on standardized tests. This trend is clearly shown for infants, toddlers, and preschool age children.

2. The operationalizing of child/staff ratios may be as important as the actual ratio. For example, ratios of 20/10, 30/10, or 40/10 are equivalent to ratios of 2/1, 3/1, and 4/1. Data show, however, that these ratios are not psychologically equivalent. Specifically, when caregivers are assigned to small groups of children (2/1, 3/1, 4/1) the consequences are different than when assignments are not made and 10 caregivers are working with variable size groups which constantly change. Further, the evidence suggests that caregiver relationships should be stable from day to day. (Recently, Lucy Conboy communicated orally to me that children and caregivers, when allowed, tend to form natural clusters; caregiver and child characteristics interact to form relatively stable groupings. Perhaps small groupings should be allowed to form in this way).

The child/staff ratios necessary for good physical care of children, but without a formal educational program, can increase and thus reduce costs. Major increases for infant day care are not possible, but ratios as high as 12/1 for 5- and 6-year-olds have not proved harmful. Again, local capabilities and values should prevail in making these determinations.

Several variables that should influence child/staff ratios were examined including: child characteristics (in addition to chronological age); setting (family-style vs. center-based); program philosophy; caregiver characteristics; and, requirements for minimum and maximum programs.

There are two especially important issues in these sections: (1) where family-style programs include older children, it is possible to use these children as caregivers themselves (similar to Skeels' procedure); and, (2) there are unresolved questions about the levels of professionalism of caregivers. Caregivers in the family-style programs should be encouraged to receive training using any one of several available parent-training programs. These caregivers should also be given incentives to maintain their programs; again, children require stable relationships. No recommendations were made about the nature of effective incentives, but money, or free materials and toys, might be feasible.

Recommendations to modify the FIDCR (pp. 65-67) regulations regarding staffing focused primarily on a perceived need to make them more flexible and realistic. The major recommended change was to clearly specify that child/staff ratios represent the actual number of children working with a caregiver rather than an overall ratio of all children to all caregivers. For example, a ratio of 5/1 means five children working with a single, and relatively constant, caregiver. A ratio of 50/10 also is a 5/1 ratio, but it does not define the working or operational group. Thus, in a 50/10 plan a few children will receive considerable care and some children will receive none. The size of the actual ratio can vary considerably depending upon children's age, the physical setting, and program philosophy. But, if the ratio is to have meaning, it seems necessary to define it in terms of actual caregiver/child contact rather than an abstraction derived from the number of caregivers and children.

Federal Interagency Day Care Requirements

The Relationship of Staffing Characteristics to Child Outcomes

Contract No. HEW-100-76-0145

Dr. William J. Meyer

This position paper is concerned with two issues which are interrelated: (1) a conceptual analysis of how children can be harmed, and (2) the effects of child/staff ratios and staff professionalism on the quality of day care provided to children in home or center settings. The discussion of these two major themes is conducted in a context of four categories of outcome variables: health and physical development, motor development, cognitive development, and social and emotional development.

Before a discussion of these themes can begin, however, it is necessary to establish exactly what day care is, to at least recognize the involvement of value judgments in any recommendations about day care, and to discuss the methodological considerations which limit the conclusions that can be drawn from research relevant to day care.

Definitions of Day Care

In its most literal definition, the term day care refers to what a child experiences during his or her waking day regardless of whether the child is cared for at home, at a neighbor's home, or at a day care center. A more common definition implies that for some portion of the day a child is cared for by a person or persons other than one of his parents or close relatives. What is important to note, however, is that the converse is also true: for a portion of his waking day the child is cared for by at least one of his parents or a close relative and his nights are spent in his home. Thus the child's parents (or close relatives) continue to play a daily significant role in his life. Therefore, day care cannot be equated with residential care, the situation in which the child is maintained in a setting away from home for prolonged periods of time, the situation where the child, in effect, is institutionalized. The major implications of this distinction are that the effects of these two types of care may differ (and thus research showing the effects of institutionalization on children cannot be assumed to show the effects of day care on children) and the requirements for these two types of care may differ.

In addition, it is important to note that day care does not necessarily include education. The idea that children deprived of the kinds of stimulation related to intellectual development might benefit from a preschool experience was translated into Project Head Start. The major aim of this program was to provide children with medical and dental care, adequate nutrition, and the cognitive and social skills necessary for success in the public elementary school. These programs were not designed, as day care was, to meet the needs of working parents or as a service for parents otherwise unable to care for their children.

A frequent question asked is whether Head Start was successful. Fein and Clarke-Stewart (1973) argue that the question is largely unanswerable, for it does not define the criteria for success. Most often success implies something about cognitive development, but a careful review of the literature fails to provide a clear picture (Hunt, 1975; White, 1973; Filipp, 1976). Aside from the many methodological problems inherent in most studies, there is the problem of finding out exactly what happened in programs. Very little information about the content of the programs is provided in the reports of these studies. In most instances investigators implicitly defined Head Start as a single program without examining what actually happened to children. Further, there is some indication that there were significant interactions between program content and the entering level of ability of the children. Thus programs that were highly structured and skill-oriented brought about greater gains with children whose entering performance was lowest. Problem- or cognitively-oriented programs seemed to be more successful with children with initially higher test scores. Perhaps the major reason that firm conclusions cannot be made is that most evaluation studies were short term; they covered periods of nine months or less. It is possible that positive (or negative) effects emerge later in the child's life.

Our intent here, however, is not to debate the merits of Head Start but rather to indicate that day care programs can exist with or without formal educational programs. While it might seem somewhat indefensible not to provide children whose risk of failure is high with any one of the readily available skill-building programs, such programs are not a necessary part of a day care program. Indeed one can imagine that some parents might not desire a formal educational component in the program for their child, and they should have such programs available to them. It will be argued later, and in more detail, that the failure to include an educational program cannot be considered as evidence that harm is being done to the children.

It is probably worth noting at this point that the programs that are available for preschool children were designed for the Head Start setting. Typically this means that the children's day is planned around a three-hour program unlike the six- to eight-hour day care program. The implication is clear: packaged programs can be used for a part of the day, but caregivers will need to know something about less formal ways in which children will learn. Care must be taken that caregivers understand that learning is always occurring and that they have a responsibility to capitalize on each informal opportunity that arises during the course of the day.

The Effect of Value Differences on Day Care

While there is some concern about the quality of care a child receives at home from his parents and the values that are passed from the parents to their child, these issues become of major importance when child care is provided by someone other than the parents (or a close relative). The concern reflects our culture's value that within broad limits parents have the right to rear their children as they wish. To the degree that parents in our culture differ in their judgments about what type of care a child should receive and what values should be inculcated in the children in our country, to that degree we must

provide only minimal requirements that reflect the widest agreement possible and allow the parents to be involved in determining more specifically the kind of day care programs their children receive.

The degree to which the quality of day care is important is reflected in the Federal Interagency Day Care Requirements (FIDCR), which were originally published in 1968 and were revised in 1972. These requirements specify child/staff ratios required for programs receiving federal support. These requirements had a perceptible impact on federally supported programs throughout the United States. The staffing specifications progress from a low child/staff ratio (few children per staff member) for infants and children three years and below to progressively higher child/staff ratios (many children to staff) for older children. It would seem that of the several requirements for federal support specified in the FIDCR, the most negative reaction focused on the child/staff ratios. A moment's reflection suggests the reason for this reaction: as the number of children per caregiver is reduced, there are concomitant increases in the costs of operating a program. Thus as costs increase, state costs and the costs involved for parents in programs supported largely by fees increase substantially. These costs occur because approximately 60% of a day care budget is attributable to personnel costs. In view of the social and economic concerns about child/staff ratio requirements, it seemed desirable to establish what <u>scientific basis</u> there is, if any, for these ratios and to determine the effects of variations on the quality of day care for children.

The degree to which value differences are important is reflected primarily in outcome variables. Outcome variables measure what the outcome or result of our rearing is, and to the degree that the people in this country do not agree on what the outcomes should be (i.e., on what a child or adult should be), then to that degree we cannot interpret measures of outcome without imposing our values on people who disagree. A good example of the effect of differences

in values is the interpretation Schwarz and Wynn (1971) made of their finding that children in day care were more aggressive than children reared at home. They interpreted this as showing a deficiency in day care rearing. Forty years ago, however, it was considered an advantage of day care that it helped children become more aggressive (Thompson, 1944). Thus, the same finding has led to different conclusions about day care. The point is that science is concerned with describing and predicting what will occur under differing definable circumstances. Whether more aggression is good or bad depends on the values of the person who considers the finding. If parents know what child characteristics result from what type of rearing, they can choose the type of rearing that fits their needs and their values. Perhaps the most serious consequence to society that can occur is if, under the guise of scientific opinion, we so regulate our programs that there is no room for variation. Since survival is a highly valued goal for almost all of us, it seems clear that we should encourage program diversity and thus, hopefully, diversity among children. However, it is also necessary, in order to encourage survival and protect our children from harm, that we do have some requirements that day care programs must meet. However, in this paper it is assumed that the fewer the requirements that can be made and the more minimal these requirements can be and still protect our children from what most of us would agree is harmful, the better, because diversity will be encouraged and imposing one set of values on people who may not agree with them will be minimized.

Methodological Considerations

One of the persistent problems in studies comparing treatment groups (e.g., different methods of teaching) is that most often the groups are not randomly assigned to treatments. Campbell and Stanley (1963) refer to such studies as "quasi-experimental designs." On the surface, nonrandom assignment does not appear to be of great consequence, but such designs frequently lead to erroneous

conclusions (Campbell & Erlebacher, 1970). If the researcher does not randomly decide who gets what treatment, then he cannot conclude that the difference in treatment groups is due to the treatment because it may be due to what it was that caused the subjects to be in that treatment in the first place. Because of these sampling biases, it is possible to find spurious support for or against hypotheses which might have serious consequences for policy decisions. An often used tactic to overcome the sampling problem is to pretest the subjects and equate the groups on the basis of the pretest. It has been shown, however, that the pretest is likely to interact with the treatments and may well be responsible for the outcome of the study. Even in those rare instances where children are assigned randomly to treatment groups, the sampling usually involves two teachers, one for each treatment. Subsequent analyses based on the children as the sampling unit are incorrect; the average of the childrens' scores (the class mean) is the appropriate measure. This means that the study consists of a sample of two: one teacher for one treatment and a second teacher for the second treatment. The reason that the classroom must be considered the sampling unit is that the outcomes for the two groups may be different because the teachers were different rather than because the treatments were different. In addition, in order to be able to assume that the treatment effects found for the two groups of children in one study will be found if the better treatment or method is applied to all children (of that age or with the same specific characteristics), it must be shown that more than the specific two groups of one study respond this way to the different treatments. Thus in experiments manipulating child/staff ratios the data have little meaning unless the studies are replicated, showing the same effects a number of times.

Because of the rather technical nature of many of the methodological issues related to the major topics of this paper, it seems best to sumarize the major problems as they emerge from the literature. Since the issues that will be

included relate to many studies, no one study will be cited.

1. In evaluating the evidence, care must be taken to differentiate day care effects per se (mother-child separation) from program or caregiver effects. With respect to aggressive behavior, for example, some writers have contended that day care children are more aggressive than similar home-reared children. This conclusion fails to consider the literature on child-rearing practices showing that aggression in the home is associated with many of the same practices that occur frequently in programs for young children. It has not been demonstrated that separation from parents in itself causes aggression.
2. The problem of sampling has already been discussed, but there is another sense in which sampling is an issue. Specifically, in many studies the samples are extremely atypical and therefore the conclusions are not readily generalizable to more typical populations. The problem is compounded by the fact that where sample comparability is approached, the results and interpretations have not been in agreement. I refer here, of course, to the studies of children reared under severe deprivation.
3. A third problem is concerned with the issue of rater bias. In none of the studies I have reviewed for this paper can it be said that the observers were unaware of the groups they were observing, and in many instances the observers were aware of the hypotheses (they were the authors of the papers). There is a real possibility that the reason many studies fail to replicate is because of the operation of observer bias.

4. One implication of the quasi-experimental design is that most studies are correlational in approach. Every undergraduate psychology student knows that cause-and-effect statements cannot be made on the basis of correlational studies — a fact frequently forgotten by scientists. A major problem arises when the scientists uses two samples selected on the basis of some characteristic (high vs. low anxiety, race, or socio-economic status) and comparisons are made between the groups after they receive some sort of treatment (day care vs. no day care). Usually the statistical analyses are in the form of an analysis of variance, which gives the impression that a formal experiment has been conducted. The problem here is not the use of analyses of variance, but the conclusion that the subject characteristic caused the outcome, or the treatment caused the outcome, or that the interaction of the two has meaning. In fact, studies of this type are correlational studies; the subject characteristic is a covariate that operated on the treatments in ways that are incapable of interpretation. Thus it can only be concluded that a particular subject characteristic correlates with one treatment to a certain degree and with another treatment to another degree (or the same degree). Statements of cause-and-effect are simply incorrect in this design.
5. An issue related to the last design problem is one raised by Bell (1968). Bell has shown that regardless of the design of a study there is a problem in interpreting the direction in which effects occur. For example, in many of the studies examining the effects of parental child-rearing practices on

the social and cognitive development of the child, it is assumed that the parents cause the variation in child behaviors; that is, parental differences cause child differences. It is equally plausible, however, that the variations in children's behaviors (temperament characteristics, for example) cause variations in parental behaviors. Research is now available suggesting that in fact child behaviors do influence parental behaviors (Moss, 1967; Osofsky & O'Connell, 1972; Stachler, 1964). A similar case can be made in terms of the effects of variations in teaching styles on the social and cognitive variations among children.

6. With very few exceptions, as already noted, studies of the effects of interventions have been short term. The possibility then exists that early intervention has latent effects that become manifest at a later stage of development. As we shall see later, some studies suggest that there are no long-term effects, positive or negative, of early stimulation or deprivation. The argument is sometimes made that the failure to find effects is a function of not measuring them at the proper time. This argument has a certain degree of validity if one accepts the latency position. However, a more careful analyses of the argument begs the question: If there is a latent effect, is there some conceptual basis for estimating when it might occur? The answer is that there is not; thus, the argument is untestable because whoever tests for latency effects and fails to find them will be criticized on the grounds that it was not the right time. In science a plausible alternative hypothesis must be testable and this one simply is not. If there is a theoretical reason to expect an

effect to occur at a certain age, then a testable hypothesis can be formed and research can be carried out. Studies that predict long-term effects at specific ages based on theory are urgently needed.

7. My review of the literature also turned up the following problems: (a) definitive conclusions based on probability (chance) values of 10% or greater; (b) separate analyses of correlated data (separate statistical tests on multiple measures which are correlated); (c) conclusions which largely disregard the data; (d) no effort to establish stability of observed behavior (reliability); and (e) a frequent failure to provide the reader with operational definitions of concepts (for instance, structured vs. unstructured parent behavior).

Outcome Measures

There are four categories of outcome behaviors which are readily identifiable: health, physical and motor development, cognitive development, and social and emotional development.

Health Outcomes

In view of the fact that a large number of day care programs provide full day care, it is clear that caregivers will play a major role in maintaining the health of the children. Sensitivity to detecting the need for medical care and a knowledge of first aid are crucial.

Physical and Motor Outcomes

It is possible that this category of behavioral outcomes is most readily measured and of greatest significance for that period of development extending from infancy through 4 or 5 years of age. Among the numerous components included under this category are the following: stages leading to walking, thumb-forefinger prehension, spatio-motor relations, perceptual-motor relations, and such complex

motor coordinations as hopping, skipping, and ball-throwing. It is probably the case that no day care program in the United States fails to provide opportunities, in one way or another, for the development of these competencies. There is some suggestion in the literature, which we will examine later, that both the pattern and tempo of the emergence of these competencies is a function of environmental stimulation, but the evidence is certainly far from persuasive. These aspects of motor development are largely unrelated to child/staff ratios among older children but certainly not infants.

Cognitive Development

There are innumerable ways in which cognitive development can be described: I.Q. scores, for example. But there are other approaches to assessing cognitive development, including those derived from Piaget's or Bruner's theory. Although there is considerable debate surrounding issues of how one best measures cognitive development, it would seem that whether one selects Measure A or Measure B there tends to be a reasonable degree of overlap. It is probably the case, however, that with respect to infant development, where measurement problems remain unsolved, that the tasks be carefully interpreted. Despite the admitted deficiencies of the several available measures, the effectiveness of day care programs will be assessed in terms of children showing normal development on those measures. It should be understood that normal development is relative to comparable groups; evaluation based on different groups makes demands on day care, or any program for that matter, that simply cannot be met. What can be confidently stated is that day care children should not decline on these measures relative to comparable samples. This argument will be elaborated upon later in our consideration of the concept of harm.

Social and Emotional Development

Of the four categories of child outcomes the category of social and emotional development is probably the one which creates the most controversy. Social

and emotional outcomes are more controversial because they reflect greater diversity in prevailing social values. Because of the diverse values about what the social and emotional development of the child should be, it is undesirable to evaluate programs and to establish requirements based on a concept of the optimum or even average outcome. For example, since people have many different values about what level of aggression is to be sought, any program evaluation that uses the concept of the desired average outcome (typically by comparing mean levels of aggression) includes a value judgment that may not be widely shared by the parents of this country. Since some requirements must be adopted, however, it is necessary that we adopt requirements that allow for great diversity; any requirements adopted should reflect widespread agreement. The greatest agreement, then, is probably not at the optimal level or even the average level of any of the social or emotional outcome variables but at the extremes of the continuum (very high or very low aggression, for example). Many people are likely to agree that any program that leads to these extremes harms the children in it. Thus, it is imperative to examine the conditions that lead to social and emotional harm.

The Concept of Harm

An inclusive examination of the psychological literature suggests that there are two broad categories of events that can harm children: directed techniques in which the caregiver is being actively harmful; general sensory deprivation which can be active but more likely takes the form of benign neglect. In this second situation it is not so much a matter of what is done to the child but a matter of what is not occurring.[1]

[1] It should be noted that there is at least a third category of harm that can occur to children which is not directly psychological in content. Specifically, this is harm that can occur to a child as a consequence of carelessness in arranging the physical setting of the environment such as broken glass on a play area, open wiring, or arrangement of furniture and other articles which can lead to tripping and consequent bodily injury. In general, these kinds of hazards can be readily remediated by thoughtful management of the day care environment whether it be center-based or family-based care.

Physical and Verbal Abuse

One of the most direct forms of harm that can occur to a child is corporal punishment, especially when it is of sufficient severity to cause physical harm. Indeed the harmfulness of such attacks upon a child by an adult is so widely recognized that there are statutes in many states which make such behavior illegal even when perpetrated by the child's parent. There are those who argue that physical punishment teaches children "respect" and who argue, in a more general sense, that the use of physical punishment is an effective technique for controlling and disciplining a child. There does not exist in the research literature any experiments which provide unequivocal support either to the position that the use of physical punishment is an effective technique of discipline or that the use of physical punishment is harmful. What little evidence exists with respect to the use of physical punishment suggests that it may have quite the opposite effect from that intended by the disciplinarian. Specifically, in a study by Sears, Maccoby, and Levin (1957), it was found that among parents who used corporal punishment as a primary technique of discipline, the children were noted by teachers for their excessive use of physical aggression outside of the home. Although a cause-and-effect statement cannot be made, there is reason to agree with the authors' conclusion that the children learn to use physical aggression by imitating their parents. Bandura, Ross, and Ross (1963), in a series of controlled experiments, lend substantial support to that conclusion. Thus it would appear that one way in which aggression can be fostered in children is through the use of physical aggression by the caregiver. The Fels Research Group (Baldwin, Kalhorn, & Breese, 1945) found that over a period of some four years, spanning ages 4 through 8, children who were consistently punished for their behavior tended to be viewed in the long term by their teachers as displaying predominantly socially maladaptive behaviors. Conversely, children reared in homes characterized as "democratic" were regarded by teachers as more

aggressive during the preschool years but more socially adaptive and mature in the elementary grades. Unfortunately, this study is confounded by the educational level of the parents; physical punishment was extensively used by the less educated parents.

It should not be concluded that physical punishment is the sole source of aggressive behavior in children. Indeed, quite the contrary appears to be the case. In the Sears et al. study it was found that parents who rarely punished aggressive behavior also tended to have aggressive children. Differences between the two groups of children emerged later. The unpunished group of children outgrew their negative aggressive behavior and were viewed by their teachers as socially integrated and mature. This result is consistent with the Fels' data. As will be seen later in this discussion, there is substantial research evidence indicating that the same effects as have been found with parent-child interactions on aggression also occur in groups. The major conclusion for now is that children who are physically punished are more likely to be physically aggressive and less socialized than children who experience different interactions with adults.

There is, of course, a plausible alternative hypothesis. As noted earlier, Bell (1968) has developed the interpretation that parental behavior can be caused by the children's behavior. More, Moss (1967) has shown that mothers are more responsive to their sons than their daughters because male babies are more fussy and demanding than females. Stechler (1964) observed the interactions of mothers and their children over a period of three years. The sample consisted of 26 families, nine of the children had suffered neonatal apnea (failed to breathe spontaneously within 3 minutes), and the remaining babies were "normal." The parents of the apnea babies were not aware of the problem. Within the first two weeks there were distinct differences in the behaviors and verbalized attitudes of the mothers of the apnea babies. These mothers

behaved more negatively and expressed strong dissatisfaction with the children than the control mothers. These attitudes prevailed throughout the course of the study. It was noted that the spina children were generally more negative and more aggressive, but most importantly, they were more troublesome from the moment they arrived home -- presumably before psychological factors could have influenced their behavior. The problem of direction of effects is clearly shown in this study. If an investigator had started a study with this sample at age 2 and did not know the medical history, the conclusion would be that the mother's negative behaviors and attitudes caused the children's negative behavior. A good case can be made from these data that the children's behavior caused the negativism of the mothers.

A more elusive type of direct harm to children is the use of verbal punishment as a technique of controlling and changing the behavior of children. Skinner (1938, 1953) has long argued that behavioral change by means of punishment technique tends to be transitory; that is, he argues there is fairly rapid recovery of the undesired behavior and many experiments have supported his argument. It would seem that if this were the only negative consequence, there would probably be little cause for concern. It is impossible to reach a decisive conclusion because the crucial experiments are not available. Again we are confronted with a number of correlational studies, including work by Meyer and Thompson (1956), DeGroat and Thompson (1949), Thompson and Hunnicutt (1944), and Meyer and Lindstrom (1973). These studies consistently show that teachers frequently use verbal reproof in their work with children. There is some inconclusive evidence in these studies that teachers use less verbal reproof with younger children than with older children. Meyer and Lindstrom, who observed 13 Headstart classrooms consisting of children between 4 and 5 years of age, however, found that the median percentage of <u>verbal reproof</u> used by

teachers was approximately 80%. Of the 13 teachers in that sample, only two teachers used more approval than disapproval.

These studies do not answer the question of whether verbal reproof is harmful and verbal praise is beneficial. These research studies do show that the children who receive the majority of verbal reproof from teachers tend to achieve lower scores on intelligence tests and achievement tests; are more likely to be males; are more likely to show signs of maladjustment; and, are typically more disruptive in class. This pattern of child characteristics is more typical of grade school children than of preschool children. It is really not possible, however, to conclude that the status of the children receiving the reproof was caused by that reproof. The studies have shown that the teacher behaviors and children behaviors occur together (i.e., are correlated), but as Bell (1968) pointed out, we do not know whether the difference in the teachers causes the children to behavior differently, or whether the difference in the children causes the teachers to behave differently, or whether there is an interaction or vicious circle occurring. Meyer and Thompson argued that teachers tend to use verbal reproof with those children who are disruptive in the classroom and thus in a sense a threat to the teacher's control. Unfortunately, as Meyer and Thompson demonstrate, verbal reproof is an ineffective means of changing the children's behavior. Good and Brophy (1974) state the problem as follows: "Apparently, unknown individual differences in the students condition the teacher into quite predictable behavior patterns, but this is done without awareness on her part" (p. 393).

The studies of teacher behavior presented so far describe what happens between teachers and children in typical classrooms. Even though these studies do not permit cause-and-effect conclusions, there does not seem to be any rationale for suggesting that the consistent receipt of verbal disapproval can help a child. Studies directly testing for negative effects, using appropriate metho-

dology, have not been done because of the ethical considerations involved (it is simply unethical to randomly assign children to situations where there is a possibility of harm). That teachers can affect children has been very nicely demonstrated by Veldman and Brophy (1974). These investigators examined the behaviors of a large number of teachers where the teachers were the unit of sampling. Some of the teachers had classes of middle-class children, but the majority of teachers were in Title I programs. The sampling procedure and the relative homogeneity of the children in the classes makes this study unique. Furthermore, Veldman and Brophy analyzed their data using appropriate and sophisticated statistical procedures (residual gain scores). Using an observational procedure developed by Brophy (1970), teacher interactions with the children were recorded and categorized according to the system. There were two major findings in the study: (1) one of the best predictors of the outcome measure (residualized gain score) was the children's pretest score; and, (2) a significant amount of variation in achievement occurred as a function of variation in teacher behaviors. The first finding is quite typical, having been reported in numerous evaluations of Head Start and Follow Through, and merely means that children who score higher initially profit more from an educational environment. (There is a situation where this outcome does not occur, and that is when the assessment instrument does not have a high enough ceiling, i.e. is too easy). Although the authors do not specify what attributes lead to the differences among teachers, this study does demonstrate with relatively little ambiguity that teacher behavior does affect achievement outcomes.

A finding of relevance to this report was that the variations in teacher style had more effects on the Title I classrooms than the middle-class classrooms. The authors make the following interesting conclusion:

> "The differences between Title I and non-Title I schools were consistent with the theoretical position that the school is relatively more important, compared to the home, in determining the achievement levels of economically disadvantaged pupils, than it is for advantaged pupils. This, in turn, suggested that the quality of teaching is more crucial in such a setting than in advantaged schools" (p. 323).

Parent-Child Separation

There are many instances in which children become separated from their parents. Some of these are tragic, uncontrollable events such as death or severe illness. Other equally tragic, but perhaps preventable, events include parent imprisonment and severe child abuse on the part of the parents. There are other circumstances, however, in which the separation is voluntary and viewed by the parents and society as having a high potential for positive outcomes. For example, Project Head Start, which took young children out of the home for at least part of the day, was viewed as a program which would foster cognitive development and better prepare disadvantaged children for public school. A more recent phenomena has been the development of day care programs. Day care programs were designed with more of a welfare concept in mind than an educational one, although such a statement is perhaps debatable. At any rate, the concept was that children who were responsibly cared for by people other than the parents would free the parents to become gainfully employed. Essentially, the concept was one of ameliorating some of the social and political problems associated with welfare programs.

A critical issue which has arisen with respect to the separation of parent and child for extended time periods (say 8 hours) is the degree to which such separation harms children. Spitz (1945) and Bowlby (1951) raised the issue and demonstrated that early physical separation from the mother had serious debilitating effects on children (Blehar, 1974). However, there is now general agreement that such effects occur only when the separation is accompanied by a severely deprived alternative environment in which the child is maintained

for an extended period of time. The instances of this kind of situation apparently do not occur with great frequency; at least there is no empirical literature available. Given the circumstances described, there can be no disagreement that harm will occur to children with this experience.

The focus in this section will be on day care experiences in which the children are separated from their parents for various periods of time. Before the available literature is examined there are certain parameters of this problem that must be made explicit in order to maximize communication.

First, there is nothing inherent in day care programs that can harm or help children. Day care is a convenient label which encompasses a number of different programs, programs that probably generate differences in children.

A second issue involves the distinction between performance and competence as applied particularly to intellectual development, but also to social and emotional behavior. Performance refers to the overt behavior and serves as the basis from which inferences about the individual's cognitive processes are made. Obviously, behavior is the only way in which process inferences can be made, but it does not follow that such inferences are always correct (accuracy as used here does not refer to conceptual issues — Piaget vs. Bruner — but rather to a procedural issue). An example may be helpful. There are items on aptitude tests for preschool children that require visuo-motor skills. These items typically require the children to copy forms such as squares, triangles, and diamonds. In addition to eye-hand coordination, these tasks demand paying attention to details (angles, perspective) and are moderately good indicators that a child is ready for formal reading instruction (although the correlation is far from perfect). When I was Director of a nursery school project I had my teachers use one of several commercially published kits for training children to draw diamonds, squares, and even octagons. These kits were used with 4-year-old children, who normally would be expected to correctly copy squares, circles,

and triangles. We were able to train the children to make any form including diamonds and octagons. Question: Did the children acquire the necessary competencies to successfully make figures on which they had not received training? The answer is no! Nor were those children able to read, or for that matter, did they know the names of the primary colors (all of these behaviors are typically associated with being able to draw a diamond). In effect we had trained a highly specific skill but had not trained anything more. Sheldon White (1965) has argued that it is possible at any age to train stimulus-response associations but that certain cognitive competencies, associated with chronological age, are required in order to develop higher levels of cognitive functioning (e.g., a higher level of abstraction required for learning reading and other academic matter). He presents a strong case that this kind of learning competency occurs between 5 and 9 years. In many cases where programs have been deemed "successful" a close examination of the content of the program reveals that the training materials were very similar, or identical to, the items on the evaluation test. More will be said about the performance-competence distinction in our discussion of language development from which the distinction was first made.

A third point of clarification needed for this section involves the meaning of such terms as structured program, open program, educational flexibility and inflexibility, and child centeredness. Somehow, in describing educational programs, or child-rearing practices, the terms open child centered, developmental, or unstructured have become synonymous with chaos, low levels of achievement caused by failure to "systematically educate the children" and with a de-emphasis on teaching "fundamental skills." Certain of these accusations are correct: skill training as a specific program activity rarely occurs (teaching preschool children to draw diamonds using one of the published training programs) and lower levels of achievement (actually this generalization applies only to pre-school children because the advantages shown by children taught specific skills

are lost somewhere around the third grade [Karnes, 1973]). The remaining criticisms are simply incorrect, or rather, reflect the difficulty in describing what happens in an open or unstructured program. Jean Carew (Carew, Chan, & Halfar, 1975) states the core of unstructured programs better than anyone I've seen so far:

> "It must be stressed that intellectually valuable experiences did not necessarily involve 'lessons' or the use of 'educational' materials. On the contrary, an intellectual experience might occur in any context of activity so long as the content of the experience related to one of the four categories previously discussed. Indeed, one of the more challenging of the observer's tasks was to be alert to intellectual experiences arising in mundane unstructured contexts which no one had planned as learning experiences and in which neither instruction nor educational toys were evident" (p. 3).

What is meant then by unstructured is that crucial learning experiences can and do occur in a multitude of settings and that the sensitive teacher must be alert to those situations. There is little doubt that more such situations occur when specific lesson plans are employed but one wonders how much time in an 8-hour day can be devoted to formalized lessons, especially with children below 5 years. In this paper it is assumed that a formalized learning experience can occur any time or any place, regardless of the lesson plan.

Essentially the same position is taken in this paper with respect to research describing parent-child interactions. Child-centered homes are not synonymous with lack of structure. The structure provided to children in these homes ordinarily involves the child's alternative solution rather than a parent-dictated solution. Confusion in the use of these terms has led to seemingly contradictory research outcomes. For example, Carew reports that children from middle-class homes of parents who are highly educated had more difficulty adapting to the demands of formal preschool settings. This result was reported by Baldwin, Kalhorn, and Breese (1945) almost 30 years ago. Hess and Shipman (1965), however, report that the mothers of lower-class children are extremely structured and

restrictive of their children. These investigators interpret their findings as indicating that such styles of mothering impede cognitive growth; that the children should be given greater freedom to explore and learn from their environments. Baldwin, et al., incidentally, report that children from highly restrictive homes, homes similar to those of Hess and Shipman's children, are well liked by preschool teachers. Thus it would appear that the multiple interpretations of terms has led to considerable confusion. In the ensuing discussion every effort will be made to provide operational definitions of these terms.

Day Care Programs

There are some people who view day care programs, without differentiation, as harmful. Specifically, it is argued, with some supporting evidence, that one of the consequences of day care is an increase in aggressive behaviors. "Specifically, taken as a whole, the evidence we have examined suggests that all day-group care may predispose children to greater aggressiveness, impulsivity, egocentrism, and related behavior patterns which have been identified as characteristic of socialization in age-segregated peer groups in America generally" (Bronfenbrenner, 1975, p. 25). This conclusion was prompted by findings reported by Schwarz and Wynn (1971), Prescott (1972), and Lay and Meyer (1973). Before examining the basis on which Bronfenbrenner derived his conclusion, it is only fair to point out that Bronfenbrenner recognizes that the phenomenon of increased aggression is probably a culture-bound concept; that is, comparative studies of peer group socialization in the Soviet Union and Israel suggest that the consequences of day care are more a function of the goals and methods employed by the caregivers rather than of day care (i.e., mother-child separation) per se!

Specifically, Bronfenbrenner is proposing that the behaviors that result from a day care program are a function of the goals of the program and thus of the behaviors that are permitted. In the U.S.S.R., for example, Bronfenbrenner (1970) has shown that young children in day care settings are quite conforming

and nonaggressive. This seemingly desirable outcome is defined by the State and is uniform across programs. Bettelheim (1969) concluded that, for the most part, the consequences of child-rearing practices in the Kibbutzim were harmful to the emotional and social development of the children. Specifically, he reports that the children seemed unable to express any deep personal feelings (love) and were also very attached to the children with whom they lived rather than to their parents or other adults. He attributes these behaviors to the failure of the children to form normal mother-child attachments, in the Bowlby sense, during infancy. Consistent with this negative interpretation, Spiro (1958) describes children reared in the Kibbutz as hostile and introverted, insecure, and overly conforming. In contrast, Rabin (1965) and Shapira and Madsen (1969) view the Kibbutz-reared child as mature, unselfish, cooperative, and highly moral. Clearly both interpretations cannot be correct. In all likelihood, all these descriptive terms are accurate, but what one reports depends upon one's values. Nevertheless, it should be pointed out, in the current context, that the experiences of Kibbutz-reared children are very different from those of Russian children. For one thing, the Russian-reared children, like children in the United States, return to their parents in the evening. In the Kibbutz there is no one figure clearly designated as the parent and the children spend only a limited amount of time with adults altogether. Thus, these programs are clearly different. Which is better? There is no answer to that question. It would seem, however, that the programs reflect the values of each society. We must determine what is consistent with our cultural values both as an antecedent to our day care programs and as outcomes.

Before a detailed discussion of the Schwarz and Wynn study, it will be instructive to review earlier day care and preschool programs in the United States. Early day care and preschool programs were largely confined to middle-class children. One of the major goals of these programs, developed in the

early 1930's, was to provide an environment for middle-class preschool children that was, from a mental health viewpoint, healthier. The view held at the time was that middle-class children, in general, were characterized by home lives that were overly restrictive and that this restriction distorted the social and cognitive growth of the child. The preschool programs which emerged had what is today called a developmental viewpoint. The developmental viewpoint is simply that children develop normally when they are given relative freedom to explore a carefully arranged environment; a concept not unlike that described by Carew. It was a positive objective of these programs to teach the children that getting dirty, messing, or playing with toys and other objects in non-stereotyped ways was permissible. Furthermore, and this is particularly relevant to the present discussion, it was felt that middle-class children should be helped to become more assertive. Operationally, the definition of assertiveness in these studies (Thompson, 1944) involved aggression — aggression which society viewed as positive: stand up for your rights, make the teacher aware that you know the answer to a question, or don't let another child push you out of line. Thus, current indictments that day care is a precursor of aggressiveness, and that this is harmful, are based on current values of the academic researchers.

That day care programs may be responsible for higher levels of aggression should be examined with considerable care. A substantial literature on the antecedents of aggression exists. It was stated earlier, for example, that aggression is correlated with the degree of structure that characterizes parent-child relations and group care (Baldwin, Kalhorn, & Breese, 1945; Sears, Maccoby, & Levin, 1957). The Lay and Meyer study is a more recent demonstration of the phenomena. For two years they observed a group of children; half of these children had been in center day care from infancy, and the other half had started day care at the age of 3 years. Their observations included several categories in addition to aggressiveness: adult-child interactions (positive and negative),

question-asking behavior, peer interactions (positive and negative), and the children's use of the center environment. In general, their data (that of Baldwin, et al.) indicate that in an open environment (an environment not counter-aggressive to aggressive behavior) there was a general increase in the output of all behaviors, positive and negative. From a psychological viewpoint, the increase in behavioral output is regarded as positive because it provides the child with a greater degree of interaction with more aspects of the environment and also provides more opportunities for the caregiver and child to communicate what behaviors are acceptable and unacceptable. The Baldwin, et al. study clearly shows a positive relationship between activity level and cognitive development. As already noted, however, children with high activity levels were generally regarded negatively during the preschool years but positively after the first grade. It should be noted that with higher levels of activity came not only higher levels of aggression but also significantly higher levels of curiosity and question-asking behavior. After the first grade the irritating activity levels became tolerable, and the curiosity and eagerness to learn was maintained. The converse of this pattern was found with those children whose parents were restrictive. (Of course, all of the generalizations are based on children in their own homes where nothing is known about the child/staff ratio).

Lay and Mayer report that their children who had previous day care from infancy also had higher levels of behavioral activity and consequent higher levels of aggression. There are two aspects of these data worth noting: (1) the increase in aggressiveness might more appropriately be labeled "assertiveness" because the early day care children asserted themselves when appropriate; and, (2) the early day care children made a much more rapid adjustment to the day care center than the children who had not previously been away from home. In view of the fact that society generally views an easy separation from mother as a sign of good adjustment (and satisfactory attachment), these data suggest that this program, developed by Bettye Caldwell and Julius Richmond (1964), had positive effects.

Operationally, the assertive behavior versus the aggressive behavior was observed in terms of significant decreases in negative aggression (hitting, fighting, or forcibly taking objects from another child). The assertive behavior was characterized by children defending their property and by not permitting other youngsters to dominate them either physically or psychologically. The Lay and Meyer findings are generally consistent with the results of numerous other studies in which less structured programs generate higher levels of general activity, including aggression (Sears, 1951; Siegel, A. E., 1956; Siegel & Kohn, 1959). These findings are also consistent with the results of the Baldwin, et al. work in which children reared in what those authors refer to as democratically oriented home atmospheres also showed higher levels of activity. If one extends the finding of higher activity levels, it follows that all classes of behaviors will increase as opposed to decreases in activity levels (and aggression) in those programs characterized by high levels of punishment and restrictiveness. Though it is true that aggressiveness does increase, it is only one of several behaviors that increase in frequency. Further, not all of the increase is negative. For example, Lay and Meyer found that their day care children were highly inquisitive and were actively engaged in constructive activities a large percentage of the time.

This brings us to analysis of the Schwarz and Wynn study which was based on the same sample as used by Lay and Meyer. The major thrust of the Schwarz and Wynn study was a comparison of children who had experienced early day care (these children were originally in Bettye Caldwell's study of infant day care) with those who had not. Their data suggest that the early day care children were more aggressive than the children without prior day care experience. They concluded, "early day care experience may not adversely affect adjustment with peers but may slow acquisition of some adult cultural values." It is unclear from the conceptualization of the study why the authors elected to

examine aggression. It is possible that other behaviors were also rated, but differences, if any, were not reported. Further examination of the study shows that Schwarz and Wynn used ratings based on relatively little observation time (one month). The raters, upper middle-class female college students, may have known which children belonged to which group. Of greater importance, however, is the fact that there is no evidence that the observers were trained to distinguish between positive and negative aggression. Finally, the differences were noted early in the program, during the first two weeks, but were not apparent during the second two weeks. This finding suggests that the differences were due to the lack of adaptation of the contrast group. These results seem consistent with the general activity hypothesis rather than any negative consequence of early day care. Certainly the very brief period in which the difference occurred does not support the interpretation that there was a day care effect. It should be noted that Levin and Turgeon (1957) found that in a controlled doll play situation children evidenced more aggressive behavior on the second exposure to the situation than on the first. Their explanation was that since the children were not initially admonished for aggression during the first session, they felt free to express aggression in the second session. However, a careful reading of the paper and of other doll play studies indicates that all behaviors increased; that is, activity levels significantly increased also. Thus, if one elected to look at some positive behavior, such as nurturant behavior, it is likely that there would have been a significant increase on that dimension. The implication is simple: in any day care setting where children are permitted some degree of freedom, there will be increases in general activity levels and therefore increases in both positive and negative behaviors. Thus, the concern that mother-child separation causes only increased aggression seems unfounded in light of more careful analysis of what is actually happening to all of the behaviors of the children. The increases in aggression

had good effects as well as bad ones: it appears that the children were acquiring skills that they will need to survive in a society where self-assertiveness is given positive value.

If, as the data suggest, a reduction in structure in the preschool is associated with increases in aggression it is reasonable to inquire about the effects of child/staff ratios and the physical facility on the incidence of aggression. The specific data on these issues are not familiar to me with the exception of the Lay and Meyer study. In that study the physical facility had been completely planned for a day care center. It had a separate dining area, a separate sleeping area, and an extremely large area divided into space for large muscle activities, creative expression (art work, pasting and cutting, and so forth), and a small room consisting of a work table and books, puzzles, and other educational materials. The very large facility, still in operation, required between 10 and 15 caregivers to adequately take care of the 40 children and also provide educational experiences. It is really not possible to specify the child/staff ratio in this facility because the children had relatively free access to areas and therefore to a number of different people.

It would seem that the Lay and Meyer facility was conducive to less direct contact with adults and therefore higher levels of activity with concomitantly high levels of aggression. Note that the behavior is not a necessary consequence of the facility. Fiene (1976) has shown that in those situations where there are few children, as in family-style day care, there is less activity and therefore less aggression. Logically then one might take the Lay and Meyer facility and assign caregivers to specific children so that the actual ratio could be, say, 4 to 1 as opposed to 40 to 10.

Family-Style Day Care vs. Center-Based Day Care

Bronfenbrenner (1975) raises several important issues contrasting the behavior of home-reared children with center-reared children. His observations

rely heavily on the work of Schwarz and Wynn and Lay and Mayor, though he also cites a study by Lippman and Grote (1974) and a study by Prescott (1973). Bronfenbrenner reports that home-reared children tend to be more cooperative, less readily frustrated by interruptions from adults, more ready to accept direction, less aggressive toward peers and adults, and "engaged in less running about as against sitting in one place." The Prescott study specifically shows that center-based children evidence more negative behaviors than children in home-based care and that children from home-based care centers are more likely to evidence these negative behaviors than comparable children reared at home. These results are also consistent with Fiene's. These results are not surprising, although the negative tone of the interpretations is somewhat troublesome. Recall that in the 1930's the great concern was with the overly restrictive environment of the middle-class home. An objective of the nursery school was, among other things, to teach children that it is all right to get dirty. One of the modern studies reports, with some alarm, that children who have had greater experience in the day care setting find it less "aversive to become dirty" than children reared at home. This statement sounds as if one can take the same bit of data, namely dirt, and use it either to demonstrate the restrictiveness of the middle-class home or the permissiveness of day care. Pick your value and "dirt" will provide the empirical support. Obviously the same bit of data cannot be used to argue both sides of the fence. This state of affairs indicates not only that we have relatively inadequate psychological theories to explain the complexities of social development but that in fact we have little adequate data on which to make sound policy decisions.

My position, a developmental position, views all of the comparative studies of home-reared children, home-based day care children, and center-based day care children in terms of the dimension of restrictiveness-openness. As programs and home atmospheres tend toward openness, there will be both more negative

behaviors and more positive behaviors. As the program or home atmosphere tends toward restrictiveness, there will be, during the preschool years, a greater likelihood of socially acceptable behaviors and fewer socially unacceptable behaviors. As Bronfenbrenner correctly indicates, there is insufficient data to indicate what long-term effects group programs or home atmospheres have on social adjustment. There is some evidence that the long-term effects of the more open programs are associated with children who are viewed by society's representatives (the teachers) as the more socially adaptive and socially desirable children (Baldwin, et al., 1945). Thus there are differences as a consequence of whether children are placed in center-based programs, placed in home-based programs, or kept at home with their mothers. At the same time there are also within-program differences of the same magnitude as those found between programs.

In summary, the issue of the possibility of direct harm as a consequence of day care programs seems at the moment equivocal. There is some evidence to suggest that center-based day care generates certain social and emotional consequences in the form of delays in acquiring adult standards of acceptable behavior. On the other hand, there appears to be some evidence to indicate that in center-based home care children develop a broader repertoire of behaviors that may well stand them in good stead when they encounter the elementary school situation. Family-based care, and particularly the basic family unit of mother and child, seems to generate fewer problems with respect to societal expectations for preschool children. Bronfenbrenner quotes Cochran:

> "The interactions which distinguish the homes from the centers were <u>cognitive verbal</u> (reading, labeling, face-to-face verbalizing) and <u>exploratory</u> in nature. The exploring in the homes involved the child's playing with objects not designed to be played with (plants, pots and pans, mother's lipstick, etc.)" (1974).

Bronfenbrenner then shows that these circumstances generate qualitative differences in adult-child interactions in family-style day care:

> "There were more instances where negative sanctions were applied in the homes than in the centers, and these instances often involve the exploring by the home or day-home child of 'no-no's' not available to children in the centers." (Cochran, 1974).

Deprivation as a Form of Harm

The second general form of harm that emerges from the literature involves the intentional or unintentional depriving of children of sensory and other forms of stimulation. Within this general category there would seem to be two classifications of research strategies. The first would be the strategy of providing one group of children with extra stimulation or specific remediational procedures. The second general strategy involves depriving the organism of a particular sensory experience. Since each of these general strategies appear to generate different outcomes they shall be examined separately and in some detail especially because of their relevance to the central theme of this paper of child/staff ratios.

Extra Stimulation

The general philosophical background for studies of extra stimulation derives from Darwinian theory and is represented in the developmental theories of G. Stanley Hall, Arnold Gesell, and Heinz Werner. This is essentially a bio-genetic view although it recognizes that environment plays a crucial role in development. Since the 1930's and 1940's more recognition has been given to the role of environmental variables. The most direct way of presenting this viewpoint is to briefly describe a well known experiment conducted by Arnold Gesell (1954). In this experiment Gesell used identical twins, thus controlling genetic predispositions of developmental rates and sequences. Gesell randomly selected one of the twins for special treatment; the experimental twin, Twin T, was given daily practice in such behaviors as creeping, crawling, thumb-forefinger prehension, and, later on, walking. This practice

was carefully designed and systematically administered with frame-by-frame photographic analysis of the child's development recorded on film. The control twin, Twin C, was afforded no special opportunities for any of these behaviors but rather was treated as a typical baby (this description might be somewhat misleading in the sense that the twins were two of several children in attendance at a day care center at Yale University which may not have been completely typical). Twin C did not receive special treatment but was kept in his crib and given things to play with and was cared for by a variety of caregivers. Presumably the only difference between Twin C and any other baby was that each week Twin C was given systematic tests in those behaviors on which Twin T was being trained. The outcome measure in this study was the age of onset of each specific trained behavior. In every test it was found that Twin C was not slower in development (indeed, the behaviors emerged in Twin C almost within the same hour as the behaviors for Twin T). These data seemed to suggest that environmental manipulations play a minor role in the development of motor abilities.

In a similarly designed study McGraw (1935) found essentially the same results as reported by Gesell. McGraw, however, extended her work to include what she called ontogenetic behaviors; that is, behaviors that she felt were largely environmentally determined but certainly were not necessary in order for one to be identified as a member of the human species. She contrasted the ontogenetic behaviors with behaviors such as creeping, crawling, thumb-forefinger prehension, and walking, which she labeled phylogenetic behaviors; behaviors that will normally occur because a person is a member of the human species. McGraw found that when the children were "ready," specific training on ontogenetic activities resulted in substantial gains. For example, when her experimental twin started to walk, she viewed this as evidence of readiness to roller skate. Using the now commonly employed technique of successive

approximations she was able to teach the experimental twin to roller skate with unusual proficiency before 20 months of age. The control twin, who was given an opportunity once a week with the ontogenetic activities, failed to demonstrate the gains evidenced by the experimental twin. The Gesell and McGraw studies seem to demonstrate that environmental intervention effects are minimal for phylogenetic activities, but greatly affect ontogenetic activities.

These studies provided the prototype for at least 200 other studies performed with both children and with animals. One of the better known studies with children was performed by Wayne Dennis (1960) with Hopi Indians. Dennis found a group of Hopi Indians who reared their infants on swaddling boards during that period of their lives when they would normally be creeping, crawling, and hitching prior to the onset of walking. In contrast, another group of Hopi Indians did permit their children complete motoric freedom similar to that of middle-class Americans. He found that the onset of walking for the Indian babies that were swaddled and the Indian babies given motoric freedom was essentially the same. Both groups, however, walked later than the typical middle-class child. Dennis concluded from this work that early environmental stimulation has only minimal effects on the onset of behavior.

The results of these studies had a phenomenal impact on child development techniques and on educational practices. As impressive as this research evidence may seem, it relies on the incorrect assumption that the control subjects were not receiving any stimulation but that "development occurred in spite of the lack of environmental stimulation." It is more accurate to say that there were no effects as a function of the <u>extra stimulation</u> provided to the experimental twin <u>above and beyond the stimulation received by the control twin</u>. What the data seem to indicate is that whatever stimulation was received by the control twins in the studies by McGraw and Gesell and by the control children in many of the other studies was <u>sufficient</u> to stimulate normal development. These

studies simply fail to make clear the nature and characteristic of the stimulation received by the control subjects that contributed to their "normal" development.

The issues involved here have been recognized by others in the field of developmental psychology. Wohlwill, for example, takes the following position:

> "Once we grant the existence of 'normal developmental processes,' that is, acting independently of particular specifiable external agents or conditions, there follows a much more far-reaching consequence. That is, that we can only hope to isolate necessary rather than sufficient causes, i.e. those without which we can assert development does not take place, rather than those thanks to which it does take place. This would suggest, in other words, that the basic tool in the experimental study of development is the deprivation study, rather than the enrichment or special study" (1973, p. 319).

Flavell (1977) makes the further observation that in so-called enrichment studies, even when they are successful, it is impossible to specify what attribute of the program contributed to growth; stated differently, it is not possible to specify what variables without which the treatment would not have worked.

Deprivation Strategy

The systematic investigation of the effects of sensory deprivation on the development of competencies received a major thrust from Hebb (1949). Hebb postulated that environmental stimulation is necessary in order for neurological development to occur at a level even approaching its maximum potential. He argued that environmental events were responsible for energizing nerves and for producing synaptic activity. As a consequence of this energizing, the neurological system was stimulated to grow and thus become more complex. He referred to the simplest of neurological networks as cell assemblies: behaviorally, a cell assembly might be represented by a simple reflex. As these cell assemblies become more frequent, they begin to interconnect. These interconnections are called phase sequences. Phase sequences continue to develop and become extremely complex and are responsible, theoretically at least, for the advanced intellectual capacity of the full-grown organism.

It is interesting to note that in Hebb's original formulations he postulated a "biochemical mechanism" that was responsible for neurological development consequent to external stimulation of the nerve endings. Recently, Rosenzweig, Bennett, and Diamond (1972) reviewed the outcome of their long-term research program examining the "biochemical mechanisms" influencing the rate of nerve transmission across a synapse. They found that, among rats, rate of learning was directly related to this biological mechanism and that the system involved is sensitive to, if not reliant upon, external stimulation. Although the bridge between the mechanism that operates for rats and the mechanisms that work for human beings is great, nevertheless, this work lends support to Hebb's general formulations and may, some day, provide important information about human neurological development.

The deprivation research strategy involves two groups, randomly selected, one of which is deprived of one or more sensory inputs. Perhaps the classic study in this area was performed by Austin Riesen (1974) in which a chimpanzee was reared in darkness from birth. If the hypothesis that environment does not play a role in the development and emergence of species-specific competencies is true, then the chimpanzee reared in darkness would not be expected to show differences from chimpanzees reared normally. Even after appropriate corrections for nerve atrophy were made, it was found that the chimpanzee was functionally blind even though the visual nervous system was intact. In another study, investigators prevented a chimpanzee from experiencing tactual stimulation by wrapping its hands in bandages. At maturity the bandages were removed and it was found that the animal was unable to perform tactual tasks as well as control animals.

There are other studies in which animals were deprived of a specific sensory input; in all cases there were harmful effects. Clearly, however, the deprivation in these studies was severe and ordinarily encompassed a major portion of the

animal's early development. As might be expected, there are no comparable studies of stimulus deprivation with children - comparable in the sense that children were randomly assigned to stimulus-deprived and control groups. There do exist, however, two major naturalistic studies in which the immediate and long-range effects of severe stimulus deprivation were examined. Dennis and Najarian (1957) and Dennis (1960) studied the development of infants and children in Lebanese and Iranian orphanages, respectively. In the first study it was found that the motor development of the infants was markedly retarded. The children were placed in the institution shortly after birth and never saw their mothers after placement. The ratio of caregivers to children was 1 to 10, and no effort was made to provide the children with any kind of stimulation, either motoric or cognitive. For example, for many months the babies merely laid on their backs, even when being fed. They were rarely held or provided with any other opportunity to acquire motor behaviors. In the Dennis study (1960), conducted in three institutions in Iran, the results were even more dramatic. Two of the three institutions can be characterized as being almost totally devoid of stimulation, even to a greater degree than was reported in Lebanon. Dennis reports that not only was the motor development of these children retarded by two to three years relative to American children, but that the sequence of development among the children was different. Specifically, when placed on the floor these children propelled themselves by "scooting" (scooting is a form of locomotion in which the child remains in a sitting position and is propelled forward by pushing with the arms and the legs). Normal children, when placed in the sitting position and urged to locomote, will turn and get on their hands and knees (feet) and propel themselves by creeping. Thus not only was development retarded but the pattern of development was also modified. Dennis argued that these findings are not the result of malnutrition or marasmus (a disease in which the body slowly wastes away) but rather a function of no

learning opportunities. The results of his data are more impressive because the infants in the third institution, a model institution, did not show the same degree of retardation nor did they evidence the atypical developmental pattern (scooting as opposed to creeping). The inclusion of this group is particularly important because it shows that the observed behavior is not a function of sampling peculiarities.

There can be little argument that the effects of institutionalization under severe deprivation conditions have serious effects on the cognitive social and emotional development of children (Casler, 1967; Yarrow, 1964). Socially these children typically show marked disturbances in dealing with interpersonal relationships. Many children are unable to distinguish between their primary caretakers and other adults and display a marked apathy not unlike that described by Spitz many years ago. That these disturbances may continue through adolescence has been suggested by Goldfarb (1949). In the cognitive domain, it is clear that performance on I.Q. tests declines steadily as a function of length of time in the institution. Language development is also severely retarded. It must be kept in mind that these consequences result from institutional experiences of the most severely deprived where no effort was made to help the children. In almost all studies mentioned in the literature reviews (Casler, 1967; Yarrow, 1964) the ratio of infants to staff was at 10 to 1. In absorbing these results the reader is reminded that these studies focused on infants and no effort was made to follow the children later in life. Thus, it is unclear from these data if the negative effects were maintained or if they could have been ameliorated by a substantial improvement in the environment. We shall now turn our attention to this crucial issue.

The question of whether early severe deprivation effects can be modified is important not only as a practical matter but also because it might help us to understand better if long-term harm can occur from less than optimal

(but not severely deprived) experiences. Not surprisingly, there are only a very few studies which can be examined because long-term studies simply do not exist. Among those that do exist there are problems of selective sampling, unspecified treatments, poor analytic designs, and problems about the adequacy of the outcome measures. As each study is examined, the salience of these issues will be noted.

I shall begin the discussion with a quotation from Dennis who compared a group of children who had been in one of his "bad" institutions and who showed signs of severe deprivation. The statement that follows is based on his observations of children between 6 and 15 years of age.

> "So far as the permanency of motor deficiencies is concerned it should be noted that Institution II had many children between ages 6 and 15 years who presumably were as retarded at ages 2 and 3 as were the children whose behavior was described above. Yet these children were attending school, playing games, doing chores, and being trained in difficult skills, such as the weaving of Persian rugs. There was nothing in their general behavior to suggest that any permanent consequences issued from the extreme retardation in motor development during the early years" (Dennis, 1960).

Essentially similar results have been reported by Kagan (1973). Kagan worked with Guatemalan Indians in two isolated and primitive settings. In one setting, San Marco, the infant is rarely allowed to crawl on the dirt floor, and there is a general belief that outside sun, air, and dust are harmful. Interactions between the infant and adults, no matter who, are infrequent. "A few with pale cheeks and vacant stares had the quality of tiny ghosts and resembled the description of the institutionalized infants that Spitz called marasmic" (Kagan, 1973, p. 950). Comparisons were made between the Indian children, American infants, and Guatemalan children reared in a city. The measures of cognitive development were especially developed to be culture-free and included measures in categories such as recognition memory, perceptual analysis, perceptual inference, conceptual inference, and fixation time. During infancy the performance of the rural infants was inferior to both the American

and the city infants. These data show the retarding effects of the rural Indian culture. Analyses of performance later in the lives of the Indian children indicated that their relative decrement had largely disappeared. Kagan and Dennis both agree that the effects of deprivation can be overcome when the child is placed in a more normal environmental setting. Kagan makes the following observation which is particularly crucial in understanding cognitive ability:

> "When educators note correctly that poor children tend to remain permanently behind middle-class children on intellectual and academic performance, they are referring to the relative retardation on the culturally specific skills of reading, mathematics, and language achievement described earlier. That relative retardation is the product of the rank ordering of scores on achievement and I.Q. tests. The fact that relative retardation on these abilities is stable from age 5 on does not mean that the relatively retarded children are not growing intellectually (when compared with themselves), but often at the same rate as economically advantaged youngsters" (Kagan, 1973, p. 958).

In yet another study by Dennis (1973), 16 children who had been adopted from the Lebanon institution were tested between 2 and 10 years later. This study is unique because of the follow-up on the <u>same</u> subjects, although there is a selective factor operating. Dennis reports that the average I.Q. of the children was 101, as opposed to their developmental quotient in the institution of 50.

The results of these studies are startling given the impoverished conditions these children encountered during infancy. As noted earlier, there may have been something special about these children so that the results cannot be generalized. On the other hand, there were three samples of children involved with essentially the same outcomes. Thus, these studies, each with its own imperfection, do replicate each other and add credence to the results. Another criticism is that all of the positive outcomes appear to be focused on cognitive and motor behaviors. We have already seen from the Gesell and McGraw studies that infants do not need a great deal of stimulation in order to develop motor

skills. What is omitted from these reports is any indication of the social and emotional status of the children in the follow-up reports. Since no mention is made about these domains of behavior, it is impossible to determine the outcomes. However, Kagan and Dennis (the first study reported) indicated that the children were making satisfactory adjustments to the demands of their environments. This suggests, to a limited degree, that the children were making satisfactory social and emotional adjustments. It might also be possible to criticize these results on the grounds that the assessments of cognitive ability were too gross to detect subtle differences. This is, of course, always possible no matter what groups we are studying or how much time has elapsed between testing periods. All that can be said is that both Dennis and Kagan used measures that have demonstrable validity and known conceptual foundations. Indeed, in the Dennis study, where the Stanford-Binet test was used, it could readily be argued that the test was culturally biased <u>against</u> the children: with a culturally more appropriate measure they would have performed even better. The most serious, and unanswerable, criticism of these studies is the fact that there is no information about the experiences the children had during the years intervening between the tests. Kagan indicates that after infancy the children are merely encouraged to participate in the day-to-day experiences provided by their culture. Dennis tells us nothing. Perhaps Kagan is correct; that is, that in order for the children in the San Marco culture to develop and acquire the knowledge necessary for a normal adjustment to <u>their</u> culture, they merely need to experience it. If this is the case, then, in agreement with White (White & Watts, 1973), the greatest plasticity (potential for learning) occurs during the toddler years.

There is yet another interpretation to these data. Before presenting it, however, it will be useful to describe two more experiments involving children in institutions for whom there were specific treatments. The purpose of examining these studies is to show that institutionalization during infancy (and mother-child

separation) need not lead to early distress and to also demonstrate that regardless of the interventions, the children develop competencies in the same order.

In the late 1930's Skeels (1966) wanted to determine if the negative effects of institutionalization could be reduced, or overcome completely, by developing a more caring environment. Because of overcrowding in the orphanage some of the children had to be moved. These children, 13 in all, were moved to an institution for the mentally retarded. A control group of 12 children remained in the orphanage. The mean pretest I.Q. of the experimental group was 64; the control group had a mean pretest I.Q. of 84. The crucial feature of the treatment program was the fact that the other children in the wards were older girls with substantially higher mental ages than the young children. Only two babies were placed in each of the wards so that in almost every case every one of the girls became attached to one of the babies to whom they devoted their energies. It was thus possible to establish an intense one-to-one relationship for each of the babies along with relationships with other interested and concerned adults. Skeels describes the situation as follows:

> "The children received constant attention and were the recipients of gifts; they were taken on excursions and exposed to special opportunities of all kinds. For example, it was the policy of the matron in charge of the girl's school division to single out certain children who she felt were in need of special individualization and to permit them to spend some time each day visiting her office. This furnished new experiences, such as being singled out, receiving special attention and affection, new play materials, additional language stimulation, and meeting other office callers" (Skeels, 1966, p. 17).

The experimental environment was indeed in stark contrast to the meager desolate environment of the orphanage.

Post-test comparisons were made over a period ranging from 21 months to as long as 33 months for the experimental group and between 20 months and 37 months for the control group. It was found that the experimental group had gained approximately 31 points, whereas the contrast group had lost approximately 21 points. In the follow-up showing what happened to these children

in adulthood, it is impressive to note that the subjects in the experimental group were living self-sufficient lives, whereas the control group subjects were almost uniformly dependent on society for their existence.

The implications of this study are clear: the negative effects of early deprivation can be reversed. It also follows that the prediction of a person's adult intellectual status is affected by all periods of development up to that time, not just by the first one and one-half years. The ability to predict long-term outcomes can be understood only by a close examination of the intervening environmental events.

The second major study was conducted by Hunt, Mohandessi, Ghodssi, and Akiyama (1976). Hunt has published numerous articles, both empirical and theoretical, concerned with the effects of deprived environments on the performance of infants on a series of Piagetian sensori-motor cognitive tasks. The citations are too numerous to mention here (see the 1976 publication for a complete listing). In general, however, Hunt has found that the more severe the environmental conditions, the poorer the children's performance. Some of the studies were conducted in an orphanage in Tehran where the performance levels of the children are substantially behind those of children in the United States as well as children living at home in Tehran. Hunt concludes that the retardation is a function of the inadequate and too few encounters the infants have with the environment. In this study, Hunt, et al. developed a two-pronged study: one study involving children in two Greek orphanages. In one orphanage the ratio of staff to children was 1 to 10. In a second orphanage, designed to be a model institution, the ratio was 1 to 3. The sample also included a sample of home-reared children. Thus this study involves a cross-sectional design; comparisons are made between the three groups of children.

The second study in this report was conducted in Tehran and involved five treatment groups, including a control group. Each of the treatment groups

are referred to as "waves" meaning that each treatment consisted of a new sample from the same population. Each sample was followed for a period of approximately 3 1/2 years or approximately the age at which the children demonstrated object permanence. The first wave of infants was the control group. The mean ages at which the infants in the group achieved each step on the Uzgiris and Hunt (1966) scales served as the standard against which the successive waves were compared. The second wave received the same human care given to the first wave but also audio-visual enrichment. The auditory enrichment consisted of taped music and taped talk by the mother. Speakers to transmit the stimuli were arranged in the cribs. Since there were numerous breakdowns, this treatment never received a fair test and will not be discussed further. The third wave received human enrichment but without instructions to the caregivers. During this program the infant/caretaker ratio was reduced from 10/1 to about 3/1 (actually the ratio was 30/10 because each of the three caretakers was equally concerned with all of the 10 infants in this wave. "The human enrichment for this group consisted of whatever came naturally for the caretakers and for the students. They got no instruction in how to foster infant development" (Hunt, et al., 1976, p. 189). The fifth wave received human enrichment in which the caretakers were given instruction in how to foster early psychological development. The infant/caretaker ratio was reduced to 2/1 or 3/1: each caretaker was responsible for only two or three infants. The caretakers were taught Earladean Badger's program for training mothers of infants (Badger, 1971a) and toddlers (Badger, 1971b). This material was supplemented with techniques for fostering vocal imitation and of semantic mastery of objects and materials in the surrounding environment. According to Hunt, et al. the Badger program, " ... teaches responsiveness to early behavioral indications of distress in infants in order to encourage their confidence that they can obtain helpful reactions from the people about them. Next it teaches recognition of the behavior signs of interest and how to distinguish these signs from those of

the boredom that comes in situations without change and ... third, it teaches them to provide play things and behavioral models that bring forth interest rather than boredom or distress."

I shall first examine the results from the cross-sectional study. The children in the 10/1 orphanage took significantly longer to achieve the highest level of object permanence, the 3/1 group took significantly less time, and the home-reared children took the least amount of time. All differences were statistically significant. An interesting, and perhaps very important, finding was the variation in performances for each of the three groups. The 10/1 group showed far greater variance than the 3/1 group. This means that in the 10/1 group some children received good care, presumably the cuter, more attractive children. When the ratio was reduced, the variation in performance among the children decreased dramatically. This indicates that the treatment was more evenly administered over children. The greatest surprise, however, is the fact that the greatest variation in performance occurred for the home-reared group. Clearly this means that home environments vary considerably in their development-fostering quality. More about this finding later. Hunt concluded the following with respect to the cross-sectional study:

> "In this cross-sectional study, the independent variable is the number of infants per caretaker. It is quite clear that the more infants for which a single caretaker is responsible, the less time she may have interacting with each individual infant. Clearly also, the higher this infant-caretaker ratio, the slower the consequent rates of development in the infants. Unfortunately, this infant-caretaker ratio is relatively unspecific from the psychological standpoint about the nature of the experiences concerned. While this infant-caretaker ratio is nicely objective and does serve to demonstrate that the environmental circumstances with which an infant interacts during his early development make large differences in the rates at which object construction and vocal imitation develop, it fails to show clearly the nature of what proximal infant experiences are important for each line and step of development" (Hunt, et al., 1976, p. 197).

The longitudinal study will now be examined. These results can best be summarized by stating that each successive wave of subjects performed better than the preceding group. The performance of Wave 5 was substantially better than that of Waves 3 and 4 and was quite impressive. Indeed, when these children were compared with 8 children from the Worcester, Massachusetts, area, their performance was superior. This is particularly impressive because the American children were upper middle-class with above average educational levels. Thus, it would appear that the treatment was very effective.

There are a number of methodological issues that can be raised about this study which would reduce the impact of the results to some degree. Most of these issues have already been stated and would serve no particular point at this time. There is, however, a broader issue which emerges clearly from one of the procedures used in this study and which must be examined if we are to ever understand the complications of intervention and assessment.

I will begin the discussion by reminding the reader of the distinction between competence and performance. It was noted then that performance is the behavior on the basis of which many educators and psychologists make inferences about underlying competence. In the Hunt, et al. study, a series of Piagetian tasks were employed. The authors were not interested in the specific behaviors measured (object permanence, for example) for their own sake but rather as indicators of the presence of an underlying cognitive competence or knowledge. Given the underlying competence, it is assumed that the child can cope with a broad variety of problems requiring the same competence. In other words, Hunt, et al. were not at all interested in specific behaviors <u>per se</u> except as indicators of competencies. (It should be noted that the literature is full of such studies but the results are so equivocal that they defy generalization). This study is important because the treatments were conducted over an

extended time period as opposed to the more frequent two-week training period. The reader should also recall that it is possible to train specific behaviors without the child acquiring the requisite cognitive competency; the behavior will appear as if it reflects the competency but it really does not affect the competency.

I shall now use an example from the Hunt, et al. study. Recall that the children in Wave 5 acquired object permanence very early, indeed earlier than a potentially high level group of contrast children. Is it possible that Hunt has found the key to fostering this important cognitive stage so that one might conceivably wish to have his own child reared in this setting? An examination of the Badger procedure, by Hunt, suggests the possibility that the training task directly trained in the behavior rather than its having been acquired more indirectly by the assimilation and accommodation of environmental stimulation. Hunt states:

> "The nature of that 'something' may reside in one of the toys. One of the child features of the Badger program for Infant and Toddler Learning consists in keeping the infants occupied with materials in which they show an interest. One of the most interesting toys in the day-care program was a shape box. By substituting a ping-pong ball for the cylindrical block supplied, these Mt. Carmel (the orphanage) infants learned almost as soon as they could sit up at five or six months of age to put the balls through the round hole and then lift the top to retrieve it. Throughout the remainder of their first year they continued to play regularly with this toy. In their play they progressed from the ping-pong ball to the cylindrical block, to the square block, to the rectangular block and hole, etc. It seems likely that it was play with this toy which served to advance object construction and hastened the achievement of the top step of Object Permanence" (Hunt, et al., 1976, p. 198).

To further underscore the possibility that the training enhanced performance but not necessarily competence, Hunt also states that whereas the Mt. Carmel children could handle the object permanence task earlier than the Worcester contrast group, in vocal imitation they were vastly behind the contrast group. This is important because, according to Hunt, who is using basic Piagetian theory, the acquisition of object permanence should be accompanied by vocal imitation. Thus, the crucial variable in the outstanding performance of Wave 5,

and the variable which probably is most responsible for differentiating them from the earlier waves, was the one toy from the Badger programs. What would have happened had the Worcester children had access to that toy?

There is an overriding issue involved in all of these studies which must now be examined. It is unfortunate that we do not now have data on the adult status of Hunt's different treatment groups, for such information would be invaluable to this discussion. In each of the studies we have examined (Dennis, Kagan, Hunt, et al.), it should be noted that all the children proceeded through the same series of stages, albeit at different rates. In the case of the Dennis and Skeels studies, where one group of children remained in the deprived environment, all the other children proceeded to achieve the various stages postulated by Piaget. This means that children achieve "readiness" for environmental challenges at different points in their developmental histories. Like McGraw's experimental twin, if the environment then makes demands consistent with readiness, important changes in performance can be achieved. In other words, it is important to recognize that the phylogenetic components of development, or the competencies, will emerge given a reasonably adequate environment. Performance, however, will be seriously influenced by the environmental stimulation; if demands are not made, then the performance characteristics may never become overt. Kagan, Wohlwill, and Flavell emphatically endorse this view and would further argue that the environment must remain drastically impoverished before harmful effects become irreversible. To give credit to Hunt, et al., they seem to be aware of this possibility, for they do report how, inadvertently, the Wave 5 treatment was so successful. Perhaps Flavell best summarizes what I am trying to say:

> "The hypothesis is that, when it comes to acquiring certain major kinds of cognition, human beings are amazingly <u>versatile</u>. They can often make do with whatever acquisitional machinery they possess and with whatever environmental content comes their way. If the usual, typical developmental route is blocked, the child may find an unusual, atypical one that somehow gets him to at least approximately the same cognitive destination."

A final observation about this issue. The work cited in this section strongly suggests that parents, preschools, and all child workers can do a better job; that is, we can reach closer to the child's maximum potential than we have done so far. But, as Hunt states very clearly, it is probably impossible to make every child a genius; there are genetic limits. Hunt does believe, and I concur, that all children can achieve when provided with an appropriate environment.

Potential for Harm
A Summary

1. Not only is the use of corporal punishment, within the limits of humanity and the law, an ineffective deterrent of negative behaviors, but it also provides the child with a model for physical aggression. Thus, it seems very clear that the use of corporal punishment is a significant risk factor in working with children.
2. The literature suggests a tendency for some teachers to rely almost exclusively on negative sanctioning behavior to both discipline and control children. The evidence further suggests that the children who receive the greatest amount of teacher disapproval are those who are most vulnerable: the less intelligent, lower-achieving, less well adjusted children. The negative sanctioning behavior is also futile, since it does not change the behavior to which it is directed. Although there is no hard evidence, it does seem highly likely that children, or adults for that matter, cannot continue to withstand such verbal abuse without some harm occurring to their self-esteem and self-concept.
3. In group settings in which child behaviors are controlled largely by negative sanctioning and fairly rigid structuring of the environment, the children have fewer opportunities for freely exploring their environment and for learning alternative modes

of behavior for dealing with both social and cognitive problems. The evidence further suggests that these children become dependent upon caregivers for direction, and indeed even learn to rely on the caregivers to define and maintain their own appropriate behavior. This latter generalization applies to preschool children (Hess & Shipman, 1965), but probably not to infants and toddlers, who appear to require more structure (Carew, 1976). The alternative, which seems more attractive and more promising for later development, is for children to assume responsibility, commensurate with their age level, for their own behavior and for determining its appropriateness. It is possible to detect when a program is overly regimented by simply observing what occurs when caretakers are out of the children's view.

4. The research evidence suggests that the failure to provide <u>any</u> adult direction and structure also fails to help children learn appropriate responses to varieties of situations. A second consequence of a totally unstructured program is the apparent high degree of aggressive and perhaps hyperactive behavior that it generates. Perhaps this is the one finding that is consistent across innumerable studies and for which it is safe to make an inference of cause-and-effect. It is not known, of course, whether or not the hyperactivity and the high levels of aggression would be maintained if such programs were generated over an extended period of time. However, one must view children's behavior in relative terms, and it is the case that the unstructured program is viewed as harmful by society because of the high incidence and intensity of aggression. It also seems likely, although not proven, that there is a higher incidence of accidents in these less structured settings as a consequence of the hyperactivity.

There is something of a paradox here in that along with the negative behaviors there is an increase in positive behaviors. For example, many child development specialists desire that children feel free to openly communicate with adults. These experts may not be as concerned with the quality of the interaction as with the fact that children feel sufficiently free and open with the caregivers. There are then advantages to the less structured situation.

5. The literature does not support the view that day care per se generates more aggression in children. It is probably the case that center-based programs, mostly because of their size, generate active children who are more likely to be aggressive. It seems quite possible and logical that center-based day care programs can be developed which provide balance between restraint and openness and which can teach children that there are alternative responses to many situations with aggression being only one but not an especially desirable behavior.

6. There seems little doubt that severe sensory deprivation impedes the course of human development. Children who are physically and psychologically abused and children who are not permitted to interact with any aspects of their environment will show severe retardation in both physical and cognitive development. The effects of early deprivation, however, are not immutable unless the child is maintained in the impoverished environment. Numerous studies showed that when the children were removed from the institution, or the programs in the institution changed, dramatic gains occurred in the children. In fact, the evidence shows that children achieve a level of development commensurate with their culture.

7. The next consideration with respect to harm involves an obvious value judgment. Some day care programs contain a substantial educational component for the purpose of preparing the children for the public school experience. Although such a component is not harmful in and of itself, the way in which the program is managed and the interpretation given to the children's "post-test" performance does raise certain considerations. In my observations of many Headstart programs, children are often brought to tears because they are forced to perform activities which, for whatever reason, are aversive to them. Typically, the aversiveness of the situation occurs because the task is too difficult for a child and the caregiver is insensitive to that fact. Observations of children in learning situations can indicate if a child is acquiring a skill or learning that "learning" is not a very pleasant activity. The latter is clearly harmful. The second concern is whether or not children show gains that are at least consistent with increases in their chronological age. Gains that do not exceed expectations should not be viewed as harmful. What should be examined is whether the children show losses. Indeed one can define harm in most cases as losses or decrements in performance from one time to another.

8. Day care programs that do not include an educational component should not be considered harmful. There is little agreement about the usefulness of such programs. Some experts believe strongly that formal educational programs are harmful. Without data, neither position can be supported. However, developmentally oriented programs are no less responsible for demonstrating that their children are maintaining their rate of development and are not in

in fact showing decrements. Because of the more elusive nature of the educative process in developmentally oriented programs, it is more difficult to detect harm. For example, one important aspect of learning about the world occurs through the play behavior of children, especially imaginary play. It is important for a child to be able to take a pencil and make it into an airplane or an automobile or to use the pencil in collaboration with some other object in some totally imaginary and seemingly creative manner. A caregiver who states to the child, "No, Johnny, that is not an airplane, it is a pencil," although possessing the best of motivations, is being totally insensitive to the fact that Johnny is developing structures and representations of the world through these activities. In effect, this behavior can be viewed as harmful; the more frequently it occurs, the more harmful the effects. A second example would be the caregiver who is insensitive to the fact that she/he is failing to communicate with a child or group of children. In my own experience I have seen this occur while reading stories to children. Very young children (3-4 years of age) have considerable difficulty relating the events in a story to anything concrete in their own world. Typically it requires a great deal of animation and often participation on the part of the child for such experiences to be successful. One can observe when such a setting is not working by the fact that children indeed pay no attention at all or become disruptive. Too much exposure to such situations will quickly turn the children away from books and similar activities and this too must be considered harmful.

The Effects of Child/Staff Ratios

The question of determining child/staff ratios can be treated from a number of viewpoints including: family- versus center-based programs, child characteristics, program philosophy, personnel characteristics, and inclusiveness (in terms of minimal standards as opposed to programs that will provide more than minimal care; educational benefits, for example). Although this report began with a list of outcome variables that should be of concern in evaluating day care, or any service for children, the discussion of harm focused almost entirely on aggression and cognitive development. This was dictated by the fact that almost exclusive attention has been given to these aspects of behavior in the research. A search of the literature, for example, failed to reveal a single study about day care that examined whether children became <u>happier</u> as a function of the program. Thus, out of necessity, the discussion will be concerned with the effects of staffing patterns on the outcome behaviors of cognition and aggression.

<u>Minimal versus Maximal Program</u>

I have selected the issue of minimal versus maximal programs as a point of departure because it pervades all the other topical variables. Furthermore, the research evidence is much more definitive with respect to this differentiation. The reader is reminded that the viewpoint taken here is that a program that does not offer a formalized educational program is not by definition harmful. It is my view that all adult-child interactions and child-child interactions have educational, or at least, cognitive implications for the child.

The work of Hunt, et al. (1976), Skeels (1966), and Dennis (1960) all indicate that the fewer children per caretaker, the greater the gains in test score performance, the greater the spontaneous verbalizations, and the less the use of aggression. The data with infants suggests that a ratio of 2/1 or 3/1, where the caretaker is assigned to the two or three children on a more

or less permanent basis, is optimal. (Unfortunately, there are no comparative studies using 4/1, 5/1, or 6/1, so this statement is really tentative). It is not the same thing when the ratio is 30/10 or 20/10 because the caregivers must spread their energies over many children. The evidence suggests that, in this situation, only a few children receive consistent educational benefits. If the purpose of a program is to provide high level educational experiences, then these ratios should be avoided. If the program is primarily concerned with providing for the physical needs of the children, the ratios of 30/10, 20/10, or 6/1 seem satisfactory.

The recommendations have so far focused on infants. There is little relevant work with respect to the toddler age group (2-4 years). In one study by Fiene, child/staff ratios were systematically varied and effects on the children's verbal behavior were examined. Although the differences in the treatment groups often did not reach acceptable probability levels for rejection of the null hypothesis, the trend of the data indicated that more adult-child verbalizations can be expected when an adult works with fewer children. In this study contrasts were made between ratios of 5/1 and 10/1; it is unfortunate that more data points were not included (6/1, 7/1) in an effort to estimate at what point the number of children per staff member critically alters the level of interaction.

In evaluating this aspect of Fiene's data one must return to the competence versus performance distinction. Given that more contact with adults is associated with more verbalization, it does not follow that these verbalizations (verbalizations are a performance attribute) are indicative of greater linguistic skill (McNeill, 1970; Chomsky, 1968; Lennaberg, 1967). It is possible that greater linguistic competence emerged from these more intense interactions with adults, but unfortunately the data are not pertinent to that question. Before one can say that the recommended ratios are necessary to adequate linguistic competence, or conversely that the 10/1 ratio will impede competence development, a more

precise longitudinal study is required. In my judgment, based on the work of McNeill, Chomsky, and Lenneberg, it is probable that a 10/1 ratio will not interfere with the acquisition of linguistic competence.

Fiene also examined differences in social patterns among the children as a function of child/staff ratios. In the 10/1 ratio condition he found that the children became more clannish; that is, they communicated more with each other than with the adults. This effect is responsible for the decrease in adult-child verbalizations but it is also an interesting phenomena in itself. Lay and Mayer found that early day care children (the Caldwell sample [Caldwell, Wright, Honig, & Tannenbaum, 1970] of day care children) interacted almost exclusively with themselves during their first year in the program. They had available to them 20 other children in addition to innumerable adults. This pattern of behavior is similar but not identical to Fiene's; it is more similar to the clannishness reported by Bettelheim (1969). The child/staff patterns in the Lay and Mayer program were on the order of 40/20 so that there was little opportunity for stable relationships among the children with a single adult. A similar picture seems to pertain in the Bettelheim situation. It would appear on the basis of these three diverse studies that there is something about the staffing patterns that influence the children's social patterns. Before drawing any hard conclusions, however, it must be noted that the early day care children in the Lay and Mayer study became totally integrated with the contrast group during the second year of the program. In addition, they began to communicate more with the adults present in the program. All of this occurred without any change in the child/staff ratios or in the program itself. We cannot estimate what would have happened with Fiene's subjects had they been studied for longer periods of time and not enough is known about the Bettelheim programs to explain the persistence of the clannishness he observed (it may have been part of the general social philosophy of the Kibbutz). Clearly, more research is needed before concluding anything about this pattern of social interactions.

Research in which child/staff ratios served as the independent variable with preschool children does not, to my knowledge, exist. Since preschool children typically range from 4 through 6 years, it might be possible to develop some clues from the staffing of kindergartens. Here again, I am not familiar with specific data but my observations suggest that a single caregiver can be successful with as many as 25 children. However, my observations are based on nursery school programs and kindergartens, not day care. Thus, it would seem that for this age group ratios of between 6/1 and 12/1 would provide a range within which an intense educational program could be provided; or at least in which the children would receive adequate care. This is at best an educated guess!

A final observation on the subject of child/staff ratios and program adequacy. The statements that have been made in this section have been based on relatively unsatisfactory data; nevertheless, they are based on data. The inferences from these data reflect my best judgment based on my knowledge of child behavior and development. There are, however, social political implications involved in these inferences that are outside of the realm of science. Specifically, whatever child/staff ratio is selected, at whatever political level, the program will cost money. If it is elected to develop a program that provides excellent physical care for the children and an excellent educational program, or even a somewhat less than excellent education program, it will be necessary to have a smaller number of children cared for by an adult. In addition to providing good physical care and cognitive stimulation, programs with small child/staff ratios also more readily permit the formation of stable relationships which appear to be important. Depending upon the age of the children, we are probably talking about ratios from as small as 3/1 to perhaps 7/1 or 8/1. If a decision is made to not emphasize an educational program; that is, to provide good physical and emotional care of the children but not a particularly strong educational

program (what I have referred to as a minimal program), then these ratios can be enlarged. I believe that they can be enlarged without any harm to the children. I don't believe that we should abandon day care because we can't afford (or won't spend) the money for optimal ratios but that we should work hard to help local communities to develop satisfactory programs at whatever ratio they can in fact afford. I believe, as does John Flavell, in the amazing resiliency of children and that they will find ways to thrive and prosper in less than optimal environments.

Family- versus Center-Based Programs

Family-style day care programs refer to programs that are conducted in the caregiver's home and are usually located in the neighborhood. Because of physical restrictions these programs are ordinarily smaller in terms of the number of children involved. It is also the case that family-style day care programs have a greater range of age levels within the program so that very often it is possible for the older children to work with the younger children providing them with both social and emotional models as well as cognitive stimulation. That such effects can happen was shown in the study by Skeels (1966). Thus, it would seem that the age mix, where it occurs, is certainly a positive feature of family-style programs.

Studies by Carew (1973) and Fiene provide evidence contrasting family-style and center-based programs. When I read these reports, it seemed apparent that both authors viewed family-style programs as being superior to center-based programs. Carew's observations indicate that there are significantly more instances of aggression, rejection, frustration, and physical pain in all-day center settings than in all-day family style settings. In addition, she reports that interactions with adults are more frequent and intense in home settings than in centers. Concurrence with these findings is reported by Cochran (1974). He found that the interactions which distinguish the homes from the centers were cognitive,

verbal, and exploratory in nature. The exploring in the home involved a child's playing with objects not designed to be played with. Cochran also reported, however, that there were more instances of negative sanctions in the homes than in the centers. These instances of negative sanctions most frequently involved the exploring in the homes of objects which the caregivers did not want the children to touch. With respect to cognitive development, Cochran failed to find any differences as a function of family-style as opposed to center-based care.

In a study reported by Bronfenbrenner (1975) the cognitive development of four groups of children was compared. This study was conducted by Winett, Fuch, and Moffatt. The four settings were as follows: (1) an all-day center; (2) family day care; (3) half-day family care; and (4) a control group (child at home with mother for entire day). The children ranged in age from 3 to 5 1/2 years and the 81 children were distributed among the four groups. The children were white, were predominantly middle-class, and had been in the given treatment for at least nine months. Since all the center programs included a cognitively-oriented curriculum, it was assumed these children would perform better on the four cognitive outcome measures. The highest performance was achieved by the children in the half-day family care treatment. The next highest mean was obtained by the children cared for at home, then the children in full-time group care, and finally the family day care group. In noting the low performance of the half-day family day care group, it should be noted that this was essentially a baby-sitting service performed by an untrained person with no intent to foster educational growth. Further, in assessing these data, differences occurred on only one instrument (it is not named) that measured "number and letter recognition, printing and counting abilities, short-term memory, and knowledge of simple addition and informational facts." Since there was no difference on the three other cognitive outcome measures (which presumably measured similar outcomes),

this study suggests no differences among treatment groups.

Drawing conclusions about the relative merits of family-style versus center-based programs is difficult because of the lack of satisfactory data. It would appear, however, that certain patterns do emerge from the findings which logically relate to the literature on child/staff ratios. Family-style programs are less likely to produce overactive aggressive children, and they are more likely to produce children who perform better on cognitive tests than children from centers. These findings are associated with smaller child/staff ratios and greater adult control of the child's activities. In this sense, the family-style program is similar to those center programs where small child/staff ratios that are stable from day to day have been established. For those parents who want to provide care for their children that comes as near as possible to that which they would receive at home, family care seems to be the answer. The evidence, in my judgment, is still insufficient to estimate whether center care is harmful relative to family care. At this time any such conclusion is unwarranted and indefensible.

Program Philosophy

The question of program philosophy and staffing patterns is one that had not been systematically investigated, especially with respect to day care programs. If the question is one of day care as a form of baby-sitting versus a combination of day care and educational services, the question has already been answered in a previous section of this paper. If it is assumed that the day care program will provide some kind of educational program, this is a more complex question because one must first determine what will happen to children developmentally in one kind of program versus another. For example, what happens to children in token economy programs as opposed to less structured developmentally-oriented programs. The evidence for this question does not really exist in the day care literature, but there are some answers from the

Head Start, preschool, and Follow Through literature. This is a vast literature at this point and cannot be reviewed here; a general summary will have to suffice (for comprehensive reviews of this literature the reader is referred to White, 1973; Hunt, 1975; and Filipp, 1976).

A review of the literature suggests the following conclusions. The effectiveness of different program approaches is a function of the age of the target population. Thus, programs that are highly structured and directive seem to be very successful with young children, especially in terms of increases in test score performance. These programs appear to influence performance characteristics more than competency as shown by the relatively early attainment of asymtote (improvement declines rapidly after kindergarten or as soon as more abstract reasoning is required). These programs are very popular among many teachers because each day's lesson plan is spelled out to the last detail, and there is obvious and visible change in the children's performance.

Programs which are less structured tend to emphasize language development, perceptual-motor skills, or are more generally concerned that children acquire "concepts" of their environments. These programs tend to fare poorly on standardized tests relative to structured programs and are viewed with a fair amount of skepticism by many teachers. In terms of my performance versus competence distinction, these programs are more concerned with developing competence and therefore are probably more sensitive to the native ability of each child. It should be noted that even though these children do not perform as well as their peers in the structured programs, they in fact evidence gains in cognitive performance; there is no evidence that these children regress.

The evidence with respect to preschool children is rather clear: structured programs are associated with higher test score performance than less structured programs. Is this advantage maintained? The answer to this question seems to be no. Karnes (1973) compared the long-term performance of five groups of children

in programs ranging from a very strict structured program through gradations to a child-centered unstructured program. During the preschool years and the first grade, the hierarchy of performance correlated perfectly with the degree of structure in the program: the more structure, the better the test scores. At the end of the second grade, the least structured programs began to catch up to the others, and the differences among the programs were substantially reduced. The final testing was performed at the end of third grade, by which time all differences among the groups had vanished. Similar findings are emerging from the ABT Associates' evaluation of Follow Through programs representing different levels of structure. In fact, the data from the ABT Study and those of Karnes seem to be a perfect replication.

What can be concluded? As noted earlier in this paper children will reach cognitive competency by different routes depending upon the environmental demands made upon them. There is no one best way to achieve competency, and the search for it will bear no fruit. The evidence also does not indicate any harm to children as a function of program philosophy, at least as defined here. It seems likely that the early success experiences achieved in the structured program provide motivational benefits to both the parent and child, but the structured program also seems to promise more than it can deliver in the long run. Less structured programs require more patience and more understanding of developmental principles. These programs also seem to require more teacher sensitivity, ingenuity, and initiative. The advantages of these programs are their breadth and their reliance upon the child to learn on his own. Since we really don't know many of the things children learn as they develop, if we teach the children those specific things we believe they need to know to such a degree that we prevent the children from learning on their own, we run the risk of narrowing their knowledge. The converse is also important. To the degree that we allow the child to learn on his own, he will probably broaden his knowledge but not

be advanced in any one area. The developmental programs attempt to chart a middle course in which they balance direct teaching with time during which the children can choose what to do. In addition, they attempt to provide an enriched environment (e.g., educational toys, scientific materials, a lot of conversation) which the children can use in their unstructured learning.

What can be stated with certainty about staffing patterns and program philosophy? Nothing! In my judgment, staffing patterns should not be influenced by philosophy. The ideal range of 3/1 to 12/1 operates regardless of the degree of structure involved. It might be that the low child/staff ratios need to be maintained for longer periods of time in the unstructured programs if it is assumed that the major (only) education that occurs is during the planned lesson. That seems to me to be a poor assumption.

Program philosophy probably influences aggressive behavior more than cognition. Specifically, structured programs seem less likely to foster aggression because the children are under more direct adult supervision. In the less structured programs the children have more freedom which fosters higher levels of activity. One of the concomitants of the high activity level is aggression. However, as Lay and Mayer have shown, along with the aggression there are increases in other more desirable behaviors, including positive adult-child interactions, more child-object interactions, and more question-asking behavior. However, since this level of aggression is viewed by some people as being undesirable, the more structured program is probably required for the children of these people. As noted earlier, family-style day care produces the least amount of aggression.

Caregiver Characteristics

In considering caregiver characteristics reality would seem to dictate that center-based and family-based programs be considered separately. The issue is that the quantity of trained day care workers, child development

specialists, and early childhood educators (especially people with infant experience) is limited.

In a private communication with David Connell, who is heavily involved in the National Evaluation of Day Care, he told me that their data suggest that a combination of low professionalism and high child/staff ratios appears to have negative outcomes for children with respect to cognitive development. Considering the issue of professionalism for the moment, logic suggests that in centers involving larger numbers of children there is a necessity for trained child specialists to administer the program and provide the other caregivers with a model of good adult-child interactions. In my experience outstanding professionals have not only understood the principles of child development and early childhood education but have also displayed considerable enthusiasm and "know how" in translating their knowledge into practice. Given that kind of professional, it is not at all clear to me how to estimate the number required within a program as a function of the number of children in the program. For example, in a day care program consisting of 40 4-year-olds, how many professional (trained) workers would be required? Another question: Is a trained specialist worth more to the children than say two partially trained caregivers? Research on this issue does not seem to be available, although Connell's data does speak somewhat to this question (more professionals seems more crucial). I doubt that there is a clear answer to these questions. The ratio of professionals to lay people is probably a function of the number of children in the center, the administrative organization of the center, and the physical layout of the center.

The issue of caregivers in family-style day care is also a complex issue. Ordinarily, the women who run these programs are from the local neighborhood and probably do not possess the level of training one finds in centers. Yet Prescott's data suggest that these children do well with respect to social

behaviors and that they perform well on various cognitive tasks. Fiene's data lend support to Prescott's data. Hunt, et al., though, working in an institutional setting nevertheless found it possible to train lay people to be very effective providers of cognitive stimulation for infants. Thus, I believe that given the relatively small child/staff ratios and an educational orientation on the part of the caregiver, positive outcomes can occur for children even though many of the caregivers are not professionals if they are supervised and taught by a professional. The problem is to determine whether the caregivers are doing more than baby-sitting because there is some evidence that children do not learn as much with just baby-sitting. A possible approach to this problem is through the use of some kind of incentive system (financial subsidy, provision of toys) for centers that have caregivers participate in training programs. Hunt has shown that the Badger programs work well, and the Office of Child Development has programs of demonstrated usefulness. In my judgment these programs should be made available, in some way, to those women conducting family-style day care.

Child Characteristics

A brief comment was made earlier that in addition to age, specific kinds of handicaps will affect child/staff requirements. It was noted in that discussion that research on how child/staff requirements must be modified to prevent harming handicapped children is not available. If there is an area where good work is needed, this surely must be it.

A less clearly defined group of children are classified as "at risk." Risk refers, usually, to the occurrence of prenatal or neonatal defects or problems. Neonatal apnea, for example, is an event that occurs at birth that can have long-term consequences (Stechler, 1964). Unfortunately, lower-class children, especially blacks, suffer higher percentages of the risk-related events: apnea, low weight for term, prenatal anoxia, and general disease (Knoblock & Pasamanick, 1953). These children frequently evidence atypical behavior which may take the

form of hyperactivity, general learning disabilities, inattentiveness, and, most broadly, behavioral problems. (The counter argument to the biological interpretation is that lower-class children do not receive the requisite training for formal educational settings and therefore make inadequate behavioral adjustments. Since white middle-class children with similar medical histories also evidence the same atypical [disruptive] behavior pattern, my conclusion is that the pattern is primarily a reflection of biological causes).

Regardless of the antecedent conditions, children who are disruptive, regardless of age, and generally hyperactive, require special care. Even in minimal care programs, these children will require more staff than other, more typical, children. In particular, their physical safety is at stake; hyperactivity can lead to serious accidents. Programs that appear to help these children educationally may require more than a single adult for each child; actual ratios will vary depending upon severity of the condition and chronological age. There are an ample number of programs available for these children, regardless of age, and these materials should be available to caregivers (see Cruickshank & Hallahan, 1974, for a review of materials and teaching procedures).

The FIDCR specifies child/staff ratios in terms of how the children are grouped (family day care home, group day care home, and day care center), and in terms of age groups. In addition, the staffing recommendations are based on what I have called a maximum program: the FIDCR <u>requires</u> an educational program for all groupings. The FIDCR also requires a trained professional for each unit. The staffing recommendations, in my judgment, are unrealistic, overly rigid in their specificity ("the child-staff ratio never exceeds 6/1"), and fail to specify that subgroups within a program should be the unit for specifying child/staff ratios (4/1 rather than 40/10).

The regulations should be made more flexible in order that local communities can afford to meet them. Although a professional child development person is

desirable, there aren't enough to meet the demand and they are expensive. Parent training programs that have demonstrable validity could be required and might be readily accepted given appropriate incentives. Similarly, the child/staff ratios never exceed 10/1 with a range from 5/1 to 7/1 for preschool age children. If an educational program is mandatory, these ratios can be justified; however, there is no sound scientific basis for selecting any one ratio. Thus, depending upon the caregivers, the children, the setting, and parental expectancies and values, ratios may need to be as low as 3/1 or could be as high as 12/1. Local programs should be permitted a range of acceptable ratios so they can determine what is best for <u>their</u> situation.

The final recommendation addresses itself to an issue not currently included in the FIDCR. Perhaps the most persuasive data included in this report are the psychological implications of defining small stable groups of children for caregivers. The current FIDCR regulations define ratios but these ratios are, or seem to be, based on unspecified groups of children and adults; it does not seem to matter how the desired ratio (5/1 or 50/10) is achieved. There is a scientific basis to suggest that children and caregivers should function in small units. If a program includes educational activities, these units (ratios) might range from 5/1 to 10/1 for preschoolers but not 50/10 or 100/10 (smaller ratios will be needed for toddlers and still smaller levels for infants). Again, the FIDCR should encourage a range of acceptable ratios but for the most effective educational and social outcomes there should be, at the very least, recommendations on how child/staff groupings are formed.

One further observation. When I accepted this task I thought it would be difficult but straightforward. It was not! The state of scientific evidence is appalling and it concerns me that such evidence might serve to determine policy which will influence children. Because of the state of our knowledge many of you will be angered by some (or all) of my inferences, conclusions,

and recommendations. Be assured, however, that I have probably anticipated your views and, in many cases, I'll be able to determine the source of your data. Because our knowledge base is so poor, I am certain that ultimate recommendations will reflect prevailing social values and this includes the question of how much we are willing to invest in our nation's youth. You may not agree with me, but I have tried to present an honest description of the data and its implications for day care. But before anyone can make unrestricted recommendations or promise great outcomes, the knowledge base must be enlarged.

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